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THE
ECLECTIC REPERTORY

AND
ANALYTICAL REVIEW,

Medical and Philosophical.

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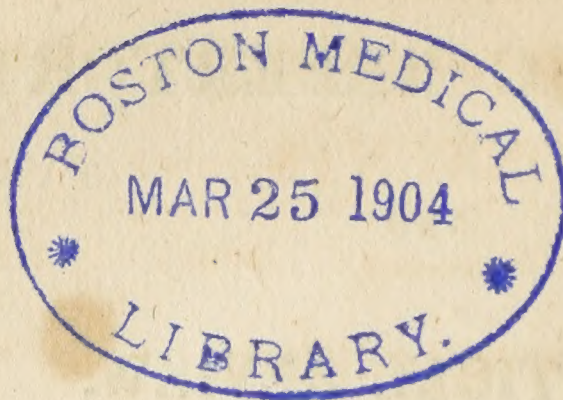
.....*Apis matinx*

More modoque.——*Hor.*

Nullis unius disciplinx legibus adstricti, quibus in philosophia necessario paremus, quid sit in quaque re maxime probabile semper requiremus.——*Cic.*

VOL. X.

PHILADELPHIA:
PUBLISHED BY THOMAS DOBSON AND SON,
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William Fry, Printer.
1820.



Eastern District of Pennsylvania, to wit:

***** BE IT REMEMBERED, that on the twenty-eighth day of October, in
* SEAL * the forty-fifth year of the Independence of the United States of America,
* * * A. D. 1820, Thomas Dobson & Son, of the said District, have deposited in
***** this office the title of a Book, the right whereof they claim as proprietors, in the
words following, to wit:

"The Eclectic Repertory and Analytical Review, Medical and Philosophical.
Edited by a Society of Physicians.

.....Apis matinae
More modoque.—Hon.

Nullis unius disciplinae legibus adstricti quibus in philosophia necessariò pa-
remus, quid sit in quaque re maxime probabile semper requiremus.—Cic.
VOLUME X."

In conformity to the Act of the Congress of the United States, entitled, "An Act for the encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies, during the times therein mentioned."—And also to the Act, entitled, "an Act supplementary to an Act, entitled, "an Act for the encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies during the times therein mentioned," and extending the benefits thereof to the Arts of designing, engraving, and etching historical and other Prints"

D. CALDWELL,
Clerk of the Eastern District of Pennsylvania.



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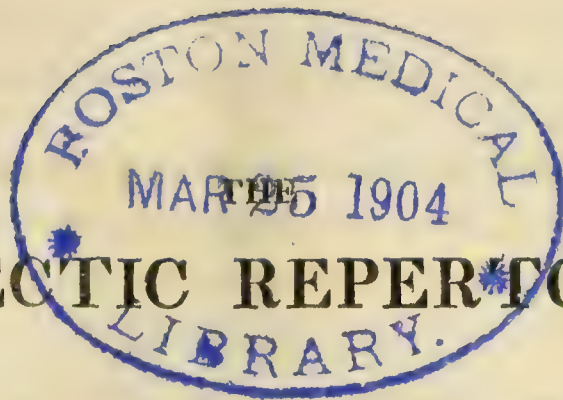
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ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

VOL. X.

JANUARY, 1820.

No. I.

SELECTED PAPERS.

Observations on the Nature und Preparation of the Elaterium.

Read at the Medical Society of London, April 24, 1819.

By DR. CLUTTERBUCK, President.

[From the London Medical Repository, for July, 1819.]

THE subject of the following observations is, with the exception of arsenic, perhaps, and the *Hydro-cyanic* or *Prussic* acid, lately introduced into practice, the most active in the catalogue of the *Materia Medica*. It is also, according to my experience, one of the most useful, and applicable to many more purposes than is generally understood. I know of no medicine, indeed, from which I have derived greater benefit on various occasions, nor one in which greater confidence may be placed. Whatever can be accomplished in the cure of diseases by active purging, may be effected, certainly, by this medicine. It is not, however, my object to point out its particular uses in the treatment of diseases; these will probably make the subject of a future communication to the society; my present intention is to make some observations regarding its preparation and doses,—points, as it appears to me, that are by no means generally or sufficiently understood. The high price of the article in the shops, leads to great and frequent adulterations, by which the practitioner is often disappointed in the effect expected from it; while, from the same cause, there is great uncertainty in regulating the dose; a circumstance not without danger: for, when

over-dosed, it is capable of producing the most violent, if not fatal, effects; especially when employed in the debilitated habits of dropsical subjects, as is frequently the case.

The only sufficient preventive of adulteration, is a reduction of price, as far as is consistent with the goodness of the article; and it appeared to me an object of some importance to inquire, whether, by any change in the mode of preparation, such a reduction could be effected? This, it is clear, could only be expected from a previous determination of the *seat* of the active principle in the plant, as preparatory to the best mode of *extracting* it. With this view, I submitted to separate examination the various parts of the plant,—the root, leaves, and stalks, as well as the flowers and fruit; and I think have obtained in a great measure the object which I was in search of. Before observing upon what had been already done on the subject, I shall briefly relate the result of my own trials.

Several of the entire plants, with the fruit or cucumbers in a state of maturity, being procured, I submitted each part in turn to trial, in order to ascertain its effects.

1. *The Root.*

The *root* being carefully dried and reduced to powder, five grains of this were exhibited to thirteen adult individuals, of different ages, and labouring under different diseases. This quantity produced no sensible effect. The dose was afterwards increased to ten grains, but equally without effect. I then gave twenty grains in nine cases; in four of which it produced no effect—one, a boy, of fourteen, it purged four or five times. The others were purged in a somewhat less degree.

As my object was not so much to inquire into the relative powers of the different parts of the plant, as whether they could be substituted in small quantities for the elaterium itself, the subject was not pursued further in this direction.

2. *The Leaves.*

The leaves were carefully dried and reduced to powder. After exhibiting several smaller doses without effect, I gave ten grains to as many different individuals, and the result was as follows:—

In *five* of the cases, no effect was produced.

In *two*, the medicine occasioned five or six stools.

In *two* others, only one.

And in *one* instance it produced no effect till the following day, when the patient was purged violently several times.

3. *The Stalks.*

When these were exhibited free from the leaves, and in similar doses, no decided effect followed.

4. *The expressed Juice of the Leaves reduced to the consistence of an Extract.*

This was exhibited in different doses from four to twelve grains, but it had no effect. An extract was made from a decoction of the leaves, but this also, as might be expected, was inert.

5. *The Petals, or Blossoms.*

These appeared to be altogether inert.

It was pretty evident from these trials, that no adequate substitute for the elaterium could be found in the plant, exclusive of the fruit; and therefore I proceeded to examine the different parts of this.

It is hardly necessary to observe, that the fruit of the wild cucumber resembles, in its appearance and structure, the common garden cucumber, differing from it merely in point of size. When full-grown, it is from an inch to an inch and a half in length, and of proportionate thickness. The seeds, which when fully ripe are of a *black* colour, are lodged in a light green pulp, the interstices of which contain from half a drachm to a drachm of nearly limpid fluid. When the cucumber is in a state of maturity, it contracts with great force upon its contents, forces open the extremity, and suddenly scatters the seeds and juice about, to the distance often of several yards. Hence the name of the *squirting cucumber*. When the time is near at hand for discharging its contents, the slightest touch will often produce the effect; and when a quantity of the cucumbers, in a nearly ripened state, are carried to a distance in order to subject them to pressure for procuring the elaterium, a great number are sure to burst on the way, more or fewer, according to the degree of ripeness, and the contained juice is consequently lost.

In order to discover in which of the parts above mentioned the elaterium, or active principle resides, I separated them carefully from each other for trial. For this purpose I slit open the cucumbers longitudinally, and allowed the clear juice to percolate through a fine sieve. The seeds and pulp were then scooped out from the interior of the cucumber, and allowed to drain through the sieve. They were afterwards washed in cold water, separated, and dried. Thus there were *four* different parts for examination; the parenchyma, or body of the cucumber; the seeds; the light pulp surrounding these; and the clear juice lodged in the centre of the fruit.

The part first examined was the parenchyma, or body of the cucumber, after the interior parts, the seeds, pulp, and juice, had been removed. The expressed juice of this part was reduced to the form of extract. Of this two grains were given in *ten* different instances; in *eight* of them, no effect followed. *One* was vomited and purged briskly, and had headach. *Another* was purged several times on the following day.

Eight grains of the same extract were then given in four cases. In two only it produced sickness and purging.

2. A quantity of the bodies of the cucumbers, freed from the juice and seeds as before, was dried and powdered. This powder was exhibited in different doses, from five to ten grains each—it produced no effect, except in one of ten instances, and this was equivocal.

3. The *seeds*, after washing in cold water, were dried and powdered. *Twenty* grains of these produced no effect; this might have been expected, indeed, for mice are observed to eat the seeds greedily.

4. The light pulp immediately surrounding the seeds was washed in cold water, and then dried. It assumed a *gummy* appearance. This likewise was inert, in doses of two and three grains.

It appears sufficiently from these trials, that the most active principle belonging to this plant, is neither lodged in the *roots*, *leaves*, flowers, nor stalks, in any considerable quantity; nor is it to be found in the *body* of the fruit itself, or in the seeds contained within it; it was only in the juice around the seeds,

therefore, that it could be looked for; and here it will be found, as is evident from the following trials.

This juice, as it first issues, without pressure, appears perfectly limpid, and colourless. After it has stood for a short time, it becomes turbid; and after some hours it slowly deposits a sediment, which, being collected and gently dried, without much exposure to light, is of a yellowish white colour, slightly tinged with *green*. When dried, it is very light and pulverulent. This is the real *elaterium*, and it is extremely active as a medicine; an *eighth* part of a grain seldom failing to purge violently, and often with vomiting previously. The quantity contained in the cucumber is exceedingly small. From forty cucumbers I obtained only six grains; so that a single cucumber contains about an ordinary dose. Half a bushel, charged at half a guinea in the market, furnished less than two drachms of *elaterium*; hence we cannot wonder at the high price it bears in the shops. This, however, is unnecessarily enhanced by the mode of preparation, and its not being known distinctly in what part the active principle is lodged.

The juice, after the *elaterium* had subsided, was evaporated to an extract. Two grains of this were given in *six* cases. In *two* of them it produced several stools; one was vomited, and was purged on the following day; *one* was vomited in half an hour without purging; and in *two* it appeared to produce no effect.

I gave five grains of a similar preparation, obtained from Apothecaries' Hall, in *fourteen* instances. In *seven* of them it produced no effect; in *three* it appeared to operate gently by stool. Of the others there was no report.

It appears from hence, that the juice, after the *fecula*, or *elaterium* has subsided, contains but very little of the active principle; and *that*, probably, from the *fecula* having imperfectly subsided.

With regard to the *sensible* and *chemical* properties of the *fecula*, which subsides spontaneously from the juice contained within the cucumber, as above described, and which alone appears to be the true *elaterium*, I have already observed, that it is a light, pulverulent substance, of a very pale green colour, approaching to yellowish white. To the *taste* it is acrid and

bitterish. The juice itself, as it escapes from the cucumber, readily inflames the skin of the fingers; and on getting accidentally into the eye in one instance, it occasioned severe pain and inflammation, with an erysipelatous swelling of the eyelids, that continued till the following day. The fecula, in the dose of one-eighth of a grain, seldom failed to produce both vomiting and purging, and that often violently. Half this quantity, viz. one-sixteenth of a grain, generally excited considerable purging. From several trials I have made, it appears to operate in little more than half the dose of that obtained from Apothecaries' Hall; which, however, is far more efficacious than what is generally sold in the shops, the quality of which varies extremely. *Some* specimens have acted very well in half grain doses; *others* have produced no effect in the dose of two grains. These are either improperly prepared, or are greatly adulterated. The best and most active I have seen, was some which I obtained from Mr. Parrott, of Mitcham, who for several years raised the plant in his own garden, and prepared the elaterium in large quantity for the Apothecaries' Company.

Much of this medicine, as it is ordinarily met with in the shops, is of a dark green colour, approaching to black: it is likewise compact and heavy, and breaks with a shining resinous fracture. This is usually prepared by strong pressure of the cucumber; and consists, of course, in great part, of the ordinary juices, as well as the elaterium. It is very uncertain in its operation, and very weak, in comparison with that which has been properly prepared. The quantity is much increased by this mode of preparation; and hence it is sold at a much lower rate. The difference in price of this drug is extreme; the Apothecaries' Company charging sometimes as much as twelve shillings a drachm, while it is sold at Corbyn's and some other shops, at *four* shillings. The quality, indeed, is superior at the Hall; but in no degree proportioned to the price. Yet I do not believe that either obtains an exorbitant profit; the difference arising chiefly from the mode of preparation, which, by the Apothecaries' Company, who adhere rigidly to the directions of the Pharmacopœia, is extremely wasteful, as I have satisfied myself by inspection.

With respect to the *chemical* properties of this substance, I shall enter no further into these, than is connected with the purposes of pharmacy.

Water, whether *hot* or *cold*, appears to have no action on pure elaterium. An infusion of eight grains, when filtered, produced no effect. Its insolubility in water, indeed, might be inferred from its spontaneous subsidence in the fluid; yet it is said, by Dr. Woodville, in his "*Medical Botany*," to be soluble both in spirit and in water. What it is that keeps the elaterium in a state of solution in the juice as first discharged, I have not ascertained.

The *elaterium* procured from the spontaneous subsidence of the juice, without expression, and which may be considered as in a state of purity, dissolves almost entirely in alcohol. Of the best specimens from the Hall, spirit dissolves more than a half; while of inferior sorts, a *fourth* part is thus dissolved. The residue, after repeated affusions of spirit, is quite inert as a medicine. The active principle therefore may be considered as of a *resinous* nature; by which I only mean, however, that it is soluble in alcohol, which it tinges of a pale green colour. When the spirit is slowly evaporated, a resinous looking extract is obtained, which is very inflammable, and which is extremely active as a medicine; the *sixteenth* part of a grain generally producing considerable purging, and often vomiting. When the dose was increased to one-fourth of a grain the effect was more considerable, and often took place in a very few minutes.

History of the Elaterium.

According to Dr. Parr*, the term elaterium was often used by Hippocrates to denote internal applications of a detergent or digestive nature. It has also been applied to any purgative that acts with violence. As applied to the wild cucumber, this substance appears to have been in use at a very early period, though subsequently in a great measure laid aside; of late years it has been restored to use: but in a limited degree, and by no means in proportion to its utility. It was at first employed as a *cathartic* merely; but in later times as a remedy for dropsy.

* "*Medical Dictionary*," article *Elaterium*.

Simon Pauli says, that from the extreme violence of its operation, it should not be used till milder means have failed. *Lister** and *Hoffman* both assert, that it produces often great heat and pulsation at the very extremities of the fingers. Others say, that it affects the head; which I have often observed to be the case. Dr. Cullen seems hardly to have used it at all; for he says, in his *Materia Medica*, Vol. II. page 544, that he had never known it employed by itself, and only as added, in a grain or two, to other purgatives. He adds, that if *Lister's* observation of its *heating* the body is well founded, he should not think of employing it at all. The *elaterium* was also occasionally employed as an errhine.

The wild cucumber plant has several synonymes; as *cucumis agrestis*; *sylvestris*; and *asininas*. The Linnæan name, which our college employs, is *momordica elaterium*. The preparation from this, which we use under the name of *elaterium*, is, in the late editions of the Pharmacopœia, called an *extract*, but with no propriety; for it is not prepared in the manner of other extracts, nor is it at all the same with *extract*, chemically speaking, for this implies solubility in water, which the *elaterium* does not possess. It is likewise often called a *fœcula*, a name applied to *starch*; but as starch is soluble in hot water, it has no analogy with *elaterium*. From the observations above made, it evidently approaches to the nature of a *resin*.

Lewis, in his *Materia Medica*, page 262, says, all the parts of the plants are strongly purgative: this opinion he appears to have borrowed from Geoffroy, who says, as quoted by Woodville, "*radicum vis cathartica major est quam foliorum; minor vero quam fructuum.*" *Lewis* observes further, that in Holland an extract from the root in wine is exhibited for the *elaterium*, and found equally efficacious. The trials which I have made, and which are described above, show that the different parts of the plant, and even the fruit itself, with the exception of the contained juice, possess very little of a purgative property, and are not at all adequate substitutes for the *elaterium*. The cucumber altogether, dried and powdered, is said,

* In Appendix to Opera Mortoni, page 25.

by Boulduc,* to be a good hydragogue: this is probable; but from what is stated above, the dose must be large.

The wild cucumber plant is an annual, and a native of the southern parts of Europe. It is very easily cultivated in common garden ground in this country, and in favourable seasons is very productive. Those practitioners who have the opportunity, would do well to raise it themselves, as the preparation of the elaterium is sufficiently easy.

Mention is made, by Berguis, of *two* kinds of elaterium, the *white* and the *black*. The *white* is prepared from the juice that flows without pressure from the fruit simply cut open. The juice is suffered to form a deposit, which is dried by the heat of the sun. It seems to be the precise substance which I have described above. *Berguis* says it is dissolved by digestion in water, but this appears to be a mistake.

The *black* elaterium is prepared from the expressed juice of the cucumber altogether, and of course contains a great deal of extractive and colouring matter along with the real elaterium. This black preparation, instead of being light and pulverulent, is said to be heavy and glutinous when masticated, and sticks to the teeth; it is irritating to the fauces. It dissolves in considerable proportion in *water*, but only partially in *spirit*. It is also deliquescent in some degree. The white is more *resinous*, the black *gummy*, in its properties.

The mode of preparing the elaterium in different pharmacopœias is far from uniform, and the medicine varies accordingly, independent of adulteration. According as more or less *pressure* is used, the medicine will approach to the *black* or *white* species, described above. It appears, from the account I have given, that pressure is not at all necessary in order to obtain the elaterium, and can only serve to deteriorate its quality, and to render the dose uncertain. It is in this way that I account for the large doses prescribed by some writers, without any caution. Sydenham recommends two grain doses without reserve; and Woodville extends the dose from half a grain to three grains. These quantities, where the medicine is in the most active form, would, I think, be often attended with danger.

* Hist de l'Academie Royale des Sciences de Paris, 1719, p. 46.

The method recommended by the London College, and which is strictly followed at Apothecaries' Hall, is the least objectionable, as it directs only the slightest pressure to be made use of. It is, however, liable to objections in other respects, from it not having been known that the elaterium resides *exclusively* in the juice lodged in the hollow of the cucumber. Hence this has been often wasted and lost. Another source of waste, and that to great amount, is the bursting of many of the cucumbers during their removal from the country.

I have obtained the greatest quantity of the medicine by pursuing the following method:—

The cucumbers should be gathered when as nearly ripe as possible, and without violence, that might endanger their bursting*. They should then be wetted by the affusion of cold water, that less of the juice when they are cut may adhere to the external surface. In this state they should be cut through longitudinally, and the juice allowed to strain through a fine sieve, placed in a large earthenware vessel. The seeds and surrounding pulp should be scooped out upon the sieve, and washed with repeated affusions of cold water, by which they will be freed from all adhering juice. Something will be saved also by afterwards rinsing the split cucumbers themselves in cold water, from which a portion of elaterium may be collected.

After standing a few hours, a sediment is formed from which the clear liquor is to be poured off; it is then to be thinly spread on fine linen, and exposed to the air to dry: a gentle warmth may be employed without injury; but the access of sunshine destroys the fine green colour which the substance otherwise acquires.

* The Dublin College direct them to be gathered before they are ripe; by this means, the chance of bursting is avoided; but it is a question whether the medicine is equally active as at a later period.

*Description of an Acid Principle prepared from the Lithic or Uric Acid. By WILLIAM PROUT, M. D. Communicated by W. H. WOLLASTON, M. D. F. R. S.**

[From the Philosophical Magazine for January, 1819.]

DURING an investigation of the principles of the urine, with the view of elucidating the pathology of that secretion, I was led to examine the well known beautiful purple substance produced by the action of the nitric acid and heat upon the lithic acid, and which has usually been considered as one of the characteristic distinctions of the lithic acid. This purple substance proved to be a compound of ammonia, and a peculiar principle having the properties of an acid;—the description of which, and of its compounds, constitutes the object of the present paper.

This acid principle may be obtained by digesting pure lithic acid in dilute nitric acid: an effervescence takes place, and the lithic acid is dissolved. The excess of nitric acid is then to be neutralized with ammonia, and the whole slowly concentrated by evaporation. As the evaporation proceeds, the colour of the solution gradually becomes of a deeper purple, and dark red granular crystals, sometimes of a greenish hue externally, soon begin to separate in abundance. These crystals are a compound of ammonia with the acid principle in question. The ammonia may be removed by the sulphuric or muriatic acid, and thus the acid principle obtained in a separate state. As, however, I found some little care requisite to obtain the acid quite free from colour, it may not be deemed superfluous to state the precise method I usually followed for that purpose. The compound with ammonia, above mentioned, was dissolved in a solution of caustic potash, and heat applied to the solution till the red colour entirely disappeared. This alkaline solution was then gradually dropped into dilute sulphuric acid, which, uniting with the potash, left the acid principle in a state of purity.

The acid principle is likewise produced from lithic acid by chlorine. Iodine has also the same remarkable property,

* From the Transactions of the Royal Society for 1818, Part II.

though in a much less striking degree. When lithic acid is boiled with iodine for some time, a partial solution of the lithic acid is effected; and if to this solution a little ammonia be added, and the whole evaporated to dryness, a perceptible quantity of the beautiful purple compound of ammonia and the new acid principle will be obtained. I am not aware that any other substance is capable of producing this change, though the circumstance is by no means improbable.

To prevent circumlocution, I shall in future call this principle the *purpuric acid*, a name suggested by Dr. Wollaston, from its remarkable property of forming compounds with most bases of a red or purple colour.

The purpuric acid, as obtained above, usually exists in the form of a very fine powder, of a slightly yellowish or cream colour; and when examined with a magnifier, especially in water, appears to possess a pearly lustre. It has no smell nor taste. Its specific gravity is considerably above that of water, though, from the minute state of division in which it exists, it usually takes a considerable time to subside in that fluid. When suffered to separate slowly from a large quantity of water, or any other fluid capable of holding it in solution, it sometimes assumes the form of thin pearly scales.

The purpuric acid is very little soluble in water. One-tenth of a grain boiled for a considerable time in 1000 grains of water, was not entirely dissolved. The water assumed a purple tint, which it retained after it was cold, though it became very slightly turbid on cooling.* The purpuric acid is insoluble in alcohol and ether. In all the mineral acids, when concentrated and in excess, and in solutions of the different alkalis, it dissolves readily; but it is insoluble, or nearly so, in dilute sulphuric, muriatic, and phosphoric acids, and also in solutions of the oxalic, citric, and tartaric acids. Concentrated nitric acid readily dissolves it with effervescence; and if the

* I am not quite sure whether the purple tint here mentioned depends upon the actual solution of a minute portion of the purpuric acid; and, consequently, whether it naturally forms a purple solution, or whether the colour be owing to the formation of a little ammonia from the decomposition of a minute proportion of the acid, which, combining with the remainder of the acid, forms the purpurate of ammonia. I incline to the latter opinion.

acid be in excess, and heat be applied, a portion of the purpuric acid is decomposed, ammonia is formed; and on driving off the excess of nitric acid by heat, the purpurate of ammonia is obtained, precisely as if a little of the lithic acid had been treated in a similar manner. Chlorine, likewise, dissolves the purpuric acid, and apparently produces the same changes upon it as the nitric acid. It readily dissolves also by the assistance of heat, in concentrated acetic acid.

The purpuric acid does not sensibly affect litmus paper, probably on account of its insoluble nature. When exposed to the air it does not deliquesce, but gradually assumes a purplish tint, apparently by attracting a little ammonia from the atmosphere, or perhaps from the evolution from itself of a little of the same alkali by spontaneous decomposition.

Submitted to heat, it neither melts nor sublimes, but acquires a purple hue from the formation of ammonia, and afterwards burns gradually, without yielding any remarkable odour. Subjected alone to heat in close vessels, it yields a considerable proportion of the carbonate of ammonia, some prussic acid, and a little fluid having an oily appearance; while a portion of pulverulent charcoal remains. When given quantities were burnt with the oxide of copper, in the manner formerly described by me,* data were obtained, which appeared to show that one hundred parts consist of

Hydrogen 4.54 corresponding with 2 atoms or proportions.

Carbon 27.27 2 ditto

Oxygen 36.36 2 ditto

Azote 31.81 1 ditto

The purpuric acid combines with the alkalies, alkaline earths, and metallic oxides. It is capable of expelling the carbonic acid from the alkaline carbonates, by the assistance of heat, and does not, as far as I have observed, combine with any other acid. These are circumstances sufficient, as Dr. Wollaston has observed, to distinguish it from an *oxide*, and to establish its character as an *acid*. On the supposition then, that it be named the *purpuric acid*, its compounds with different bases must be denominated *purpurates*: on some of the

* See Medico-Chirurgical Transactions, vol. viii. p. 526.

most remarkable of which I shall now proceed to make a few remarks.

Purpurate of ammonia. This salt crystallizes in quadrangular prisms, which, when viewed by transmitted light, are transparent, and of a deep garnet red colour; but by reflected light, their two broadest opposite faces appear of a brilliant green, closely resembling that of the wings of some of the beetle tribe, as for example, of the *Cetonia aurata*, while their other two opposite faces appear of a dull reddish brown colour; or, if the light be very strong, slightly green. This peculiarity seems to be possessed in a greater or less degree by all the other alkaline, and perhaps earthy salts; and doubtless depends upon the structure of the crystals. The purpurate of ammonia is soluble in about 1500 parts of water at 60° , but in boiling water is much more soluble. The solution is of a beautiful deep carmine, or rose red colour. In pure alcohol and in ether, it is little if at all soluble. The aqueous solution has a slightly sweetish taste, but no smell. By adding this aqueous solution of the purpurate of ammonia to neutral saline solutions of other bases, most of the following purpurates were formed.

Purpurate of potash. When a saturated boiling solution of the purpurate of ammonia is added to a solution of the bi-carbonate of potash, a dark brownish red precipitate takes place, which is the purpurate of potash. If, however, this salt be slowly formed, it may be obtained in a crystalline form; and the crystals appear to possess the same peculiarity with respect to colour, as those of the purpurate of ammonia above mentioned. This salt is much more soluble than the purpurate of ammonia.

Purpurate of soda. This salt, when obtained by the same means as the purpurate of potash, is of a dark brick red colour. It may, however, be obtained in crystals. It is much less soluble than the purpurate of potash. Three thousand times its weight of water at 60° did not completely dissolve it. The colours of the solutions of this salt, and of potash, differ slightly from one another, and also from that of the purpurate of ammonia; but it is not easy to describe these differences so as to render them intelligible.

Purpurate of lime. This salt, when obtained by adding a boiling saturated solution of the purpurate of ammonia to a solution of the muriate of lime, exists in the form of a powder much resembling in colour the crust of the lobster before it is boiled. This salt is but little soluble in cold water; but in boiling water it is more soluble, and the solution is of a beautiful reddish purple colour.

Purpurate of strontian. This salt obtained as above, with the nitrate of strontian, exists in the state of a dark brownish red powder, with a slight tinge of green. It seems to be more soluble than the purpurate of lime, and forms a purple solution.

Purpurate of barytes. Obtained as before described, with the acetate of barytes, this salt assumes the form of a dark green powder, not apparently differing much in point of solubility from the purpurate of strontian; and forming, like that salt, a purple solution.

Purpurate of magnesia. This is a very soluble salt. Its solution is of a beautiful purple.

Purpurate of alumina. When a solution of the purpurate of ammonia was added to a solution of alum, no perceptible change took place immediately; but after some time the colour of the solution disappeared, and a small quantity of a white substance separated, which was presumed to be the purpurate of alumina, but it was not examined.

Purpurate of gold. When a solution of the muriate of gold is dropped into a solution of the purpurate of ammonia, the colour becomes yellowish, but no precipitation takes place. Hence, this salt may be presumed to be very soluble.

Purpurate of platina. The muriate of platina changes the colour of the purpurate of ammonia to a yellowish scarlet, but produces no precipitation.

Purpurate of silver. Solutions of the acetate or nitrate of silver, dropped into a solution of the purpurate of ammonia, produce a deep purple precipitate; and the water is left nearly colourless. Hence the purpurate of silver appears very insoluble.

Purpurate of mercury. A solution of the proto-nitrate of mercury produces, with the purpurate of ammonia, a beautiful

reddish purple precipitate, and the water is left nearly colourless. A solution of the oxymuriate of mercury produces at first no change; but after some time a copious light rose-coloured precipitate occurs, and the solution is left colourless.

Purpurate of lead. A solution of the nitrate of lead, dropped in a solution of the purpurate of ammonia, renders it of a rose red colour; but no precipitation takes place.

Purpurate of zinc. A solution of the acetate of zinc produces with the purpurate of ammonia a solution and precipitate of a beautiful gold yellow colour; and a most brilliant iridescent pellicle, in which green and yellow predominate, forms on the surface of the solution.

Purpurate of tin. A solution of the muriate of tin changes the purpurate of ammonia to a scarlet; but this rapidly disappears, and the solution becomes colourless. After a few hours, white pearly crystals form in abundance, which is the purpurate of tin.

Purpurate of copper. A solution of the acetate or sulphate of copper changes the purpurate of ammonia to a bright yellowish green colour, but produces no precipitation.

Purpurate of nickel. The nitrate of nickel imparts to the purpurate of ammonia a greenish tinge, but produces no precipitation.

Purpurate of cobalt. The acetate of cobalt changes the colour of the same salt to a pale scarlet. After some time, reddish granular crystals form, which are the purpurate of cobalt.

Purpurate of iron. A solution of the green sulphate of iron changes the colour of the purpurate of ammonia to yellowish red, but produces no precipitate.

Such is a very brief account of the *purpurates*, as far as I have examined them. It may at first sight appear singular, that such an insoluble acid should form so many soluble compounds; but when we reflect upon the subject, and consider what a very small quantity of the purpurate of ammonia is retained in solution by water, and that this small quantity has been made the standard of comparison in the above experiments, our surprise is considerably lessened, and we feel no difficulty in conceiving, that if the purpurates were compared

with the nitrates, for example, the former would be found by far the least soluble.

From the very small quantities on which I have been obliged to operate, and from other circumstances, I can offer but little respecting the constitution of the purpurates. Those which I have attempted to analyse appear to be anhydrous, and to be composed of two atoms of the acid, and one of the base; and if this be correct, the same composition may perhaps be referred to most, if not all the compounds above mentioned. The purpuric acid, however, appears capable of forming subsalts and supersalts, with most bases, many of which seem to be very little soluble.

With respect to the characteristic properties of the purpuric acid, I apprehend it may be readily distinguished from all other substances by the beautiful colours exhibited by its alkaline and earthy salts, independently of its other properties, which are likewise peculiar.

The purpuric acid and its compounds probably constitute the basis of many animal and vegetable colours. The well known pink sediment, which generally appears in the urine of those labouring under febrile affections, appears to owe its colour chiefly to the purpurate of ammonia, and perhaps occasionally to the purpurate of soda. Some of the purpurates, as for example that of lime, might be probably used as a paint. They might be also used for dyeing, especially wool and other animal productions.* On this part of the subject, however, as I have little that is certain to offer, I do not deem it prudent to enter at present.

*I may here observe, that the solution of lithic acid in nitric acid has the property of tingeing the skin and other animal substances in a very permanent manner. The colour does not, in general, appear till the substance has been exposed to heat, or, what is more effective, to the light of the sun. In the latter case, particularly, a deep purple tint soon makes its appearance, and the substance tinged (more especially the skin) emits during the process a strong and peculiar smell, closely resembling that produced by the nitrate of silver, when applied to the skin, and exposed to similar circumstances.

On the Euphorbia Corollata.

[From the Baltimore Federal Republican, of November, 1819.]

IT is with pleasure that I now present the medical profession, with the history and medical virtues of a plant that has long since been noticed by writers on botany as belonging to the class Dodecandria and order Trigynia and the natural order Tricoccæ; though its medical properties have never as yet been mentioned by any writer on *Materia Medica*.

EUPHORBIA COROLLATA.—This plant, which is exclusively a native of the United States, is known by a variety of appellations, such as milk-weed, snakes' milk, ipecacuanha, Indian physic, &c. The country people tell us, that snakes feed upon the milky substance which pours out of every part of the plant from its receiving the slightest wound; and from this circumstance it has received the name of snakes'-milk; but ipecacuanha would appear to be the most appropriate name, inasmuch, as this word, in the language of South America, signifies vomiting root, and is given to every plant that possesses emetic powers to any considerable degree.

This valuable indigenous production may be procured in the State of Maryland, within about ten miles from this city, in any quantity. It grows in the greatest abundance in the dry and sandy soil of Anne Arundel county; it is also to be met with, in almost every other county within the state, but not in such abundance. It is never eaten by animals.—In some parts it is supposed to be a poisonous plant; while in other parts of the country it is used as an emetic in cases of *Febris Intermittens* and *Dropsy*, and, it is said, with decided benefit.

Description of the Euphorbia Corollata.

Root perennial, branching in various directions. Stem erect round and smooth, and more or less of a purple color. Universal umbel five-cleft, primary partial umbel three-cleft, the others dichotomous. Universal involucre five or six leaved, leaflets oblong, obtuse; partial three-leaved, the rest two-leaved. The rays of the primary umbel have a leaflet and a small

dichotomous branch in the middle. Stem leaves lanceolate, very obtuse. Petals, snow white, spread out flat, in fours and fives, not shaped like a petal, but very slender.

MEDICAL VIRTUES.—I have given the powdered root of the *Euphorbia Corollata* in a number of cases, by way of experiment, in order to satisfy myself of its application as a medicine; and have made use of it in practice for some time past; and I am bold in saying that I believe it to be equal to the *cephaelis ipecacuanha*. As an emetic, it is mild and certain in its operation, rarely occasioning pain or spasms, and exciting little previous nausea or giddiness; possessing an advantage over some other medicines of this class, that when it does not prove emetic, it passes off by the bowels.

The dose of the powdered root is from fifteen to twenty grains; but it may be given in larger doses, without its being attended with any violent effects. In this respect it possesses an advantage over the *euphorbia ipecacuanha*. Its activity is very little impaired by boiling. Combined with opium, in the form of the *Pulvis Doveri* it is a valuable diaphoretic. The watery and alcoholic extracts may be given in the dose of from five to eight grains; they differ very little in the force with which they act. The *Vinum Euphorbiæ Corollatæ* acts with as much certainty as the *Vinum Ipecacuanhæ*; the manner of preparing it is as follows:—℞ *Radicis Euphorbiæ Corollatæ*, unciam j. *Vini Hispani Albi*, octantem j. The dose is an ounce for a grown person.

CHEMICAL ANALYSIS.—By separating the epidermis and corticle from the ligneous part of 893 grains of the recent root, and weighing them accurately, the result was as follows: Epidermis 84 grains, ligneous or woody part 189, and 620 of cortex, or bark.—So from the above examination, it appears that the root is made up of nearly two thirds of the most active part, which is the bark.

From 2,160 grains of the recent root, I obtained 102 grains of watery extract; and from the same quantity of root, after digesting it a suitable time in alcohol, and filtering, and carefully evaporating it to a proper consistence, I obtained 123 grains of alcoholic extract.

WILLIAM ZOLLICKOFFER, M. D.

On the Language of Complaint during Pain, and the Consequences of suppressing it, principally from the French of
 BARON LARREY.

[From the Medico-Chirurgical Journal, for July, 1819.]

THE life of man, from the instant of his birth till the moment of his dissolution, is one continued struggle against the innumerable dolorific agents that surround and annoy him. The same organs of sense by which he enjoys pleasure, are the media through which pain is also conveyed. That the sense of pain is as wisely implanted in our nerves as the sense of pleasure, there can be little doubt; but our present object is to inquire whether that peculiar language of complaint, which, indeed, is the universal language of all animated nature, has any *salutary* tendency, or is merely the index of our sensations.

The infant cries the moment it enters this world; no doubt from a sense of pain occasioned by the new impression of the atmosphere. It has no other language to make known its pains or uneasiness; but in this view alone, it is a wise gift of the Omnipotent Creator. Were it not for this animated language of Nature, the careless nurse might implant pins into the infant's flesh, or neglect to supply it with sufficient aliment. As nature seldom adapts a single mean to a single purpose, we have every reason to believe that the *moderate action of crying* has a salutary influence on the pulmonary system of the child, and through that on the whole corporeal fabric.

That the language of complaint is useful in *moral* afflictions, we have the most demonstrative evidence. The fact is so striking as to have arrested the attention of the poets as well as the physicians of every age. A flood of *tears* instantly relieves both mind and body! The cheeks of Niobe were dry when grief burst her heart for the loss of her children. The observant physician sees, at every step of his professional rounds, the morbid corporeal effects of *suppressed* mental emotions. This did not escape the penetrating eye of Shakespeare.

“ She never told her love,
 But let Concealment, like a worm i' the bud,
 Feed on her damask cheek.”

It is in this way that the confidence of a friend, to whom we can unbosom our sorrows, often prevents that *corporeal* derangement in the brain, which ends in insanity or suicide, when the cause of grief is kept locked up in the breast. It is a certain fact, that even *solitude* confers a comparative relief, because we are there, as it were, enabled to give vent to our emotions in sighs, groans, or ejaculations. But when in the midst of company, to whom we dare not communicate our thoughts, we endure the pangs of the damned, and are always eager to get away into the open air by ourselves.

The ravages which grief and other melancholy emotions make upon the function and structure of our corporeal organs, is truly astonishing. They are not much less numerous than those from *physical* causes. And why need we wonder at this, when we have so many well authenticated instances of death itself, on the spot, from mental emotions? Marcellus Donatus relates, that at the siege of Buda, a young soldier fought with such surprising valour, as to excite the admiration of the whole army, both friends and enemies; but who at length fell, overpowered by numbers. It was eagerly inquired who this valiant youth could be; and after the battle, his body being found, and his vizor taken off, Raisciat of Suabia recognized him as his son! The father remained perfectly motionless for a few seconds, his eyes tearless and fixed on the corpse—he then fell over the body, and instantly expired without uttering a groan!

Innumerable instances of the effects of sorrow, though less suddenly fatal, on the functions and structure of the viscera might be here adduced; but we shall only state one or two. Boyle relates that a woman having sat down to rest on the bank of a canal, permitted her infant child to creep about on the grass. The child however fell into the water, and the mother on looking up saw her offspring sinking! She was instantly stricken with palsy of one arm, which continued during life.

But what intelligent or experienced physician has not seen hypochondriasis, mania, epilepsy, result from moral afflictions? It is, however, on the *epigastric centre* that grief and anxiety chiefly vent their morbid effects. Hence it is that

aneurism and other structural derangements of the heart are so frequently developed; and hence too those scirrhus affections of the stomach, especially of the pylorus, which we meet with among people long preyed on by melancholy emotions of the mind.

The hepatic system also comes very particularly under the influence of suppressed sorrow, and strong mental conflicts. We have seen an incredible number of these cases. Renaudin relates the case of a man who, in one minute was suffused completely yellow, from mental agitation, under his own eye. But to return to the language of complaint in painful diseases, and during operations.

The proud stoic will tell you, that as pain is not an evil, to complain is unworthy of a man; and to *cry* is a piece of shameful pusillanimity. But the stoic, instead of being a natural, is a most *unnatural* philosopher! Pain is the lot of human nature; but the language of complaint is permitted to him who suffers. Pain, indeed, very generally tames the proudest and the fiercest mind; like the force of love, it reduces all ranks to a level. When Felix operated on Henry the Fourth, of France, for a *fistula in ano*, the patient at first endeavoured to suppress his feelings, and act the monarch. But the bistoury soon subdued his resolution, and before the operation was over, this prince and hero roared as loud as any of his subjects!

To *moderate* the expression of our feelings in painful diseases, and especially under surgical operations, is commendable; to *suppress* them is dangerous. It is not beneath the dignity of the bravest warrior to wince under the surgeon's knife. Read the Iliad, and your ears will be stunned with the cries of the Grecian and Trojan heroes when wounded. In fact, the *experienced* surgeon will be far from gratified to find his patient utter not a complaint during a painful surgical operation. It is a bad omen, whether it result from insensibility of the patient, or a proud suppression of his feelings. Anne, of Austria, was afflicted with a cancer in the breast, which caused the most excruciating torture at intervals. Her confessor exhorted her to restrain her expressions of sufferings at these times; but in doing so, she experienced such a sense of suffocation and anguish internally, as nearly overwhelmed her.

The Marechal du Muy was operated on for stone in the bladder. He prayed devoutly at mass, just before the operation, for strength of mind to bear the pain; and he so far suppressed the feelings of nature, that he uttered not a murmur while the stone was extracted. He died almost immediately afterwards.

Baron Percy, who has performed as many surgical operations as any man living, remarks, that "it is a favourable circumstance when, under great operations, the patient cries and shed tears. It appears that, in such cases, a kind of general relaxation takes place in the system, rendered previously almost convulsed by the pain. The functions of the heart are deranged; the respiration is impeded; the blood is detained in the large vessels; the nervous system is spasmed; and the motions of the diaphragm are irregular and tumultuous. All these morbid phenomena are greatly exasperated, *if the language of complaint be suppressed*; si le malade se tait et résiste. The impression of anguish cannot be effaced afterwards; the vital powers take a destructive direction; the whole animal economy is perverted; and sudden death, or a low fever, too often ending in the same, is the sad result of this state of things." "L'empreinte de la douleur ne peut plus s'effacer; les propriétés vitales recoivent une direction destructive; L'organisme entier est entraîné dans cette perversion, et une mort prompte, ou une fièvre ataxique dont elle est trop souvent la terminaison, est le triste résultat de cet état de choses." "If, on the contrary, the patient cries out; if he exhales; if, as Montagne says, he evaporates the pain, these effects will not take place, and these impressions will be but transitory. "Si le malade crie, s'il exhale, si, comme dit encore Montaigne, il évapore sa douleur, ces effets sont presque nuls, et ces impressions simplement fugitives." Every cry, which consists in a deep inspiration followed by a sudden and interrupted expiration, dilates and distends every part which the pain had previously constricted; thus preventing congestions; facilitating the circulation of the blood through the lungs; disembarassing the heart: in fine, bringing constantly back that *order*, which pain, the great enemy of order, tends incessantly to subvert.

The Baron remarks, that he has often met with men who, in the midst of the most terrible sufferings, knew not how to, or could not cry out. They appeared as if stupified, or absorbed by the pain, which seemed to reign paramount over every other sentiment and instinct. "I have, says this experienced surgeon, exhorted men, under these circumstances, to cry aloud, when I saw the pain ravaging on their vitals; their necks and chests swell; the hair bristle on their heads; the mouth closed, and distorted; all the muscles contracted; the face pale, and sunk; the eye fixed and projected—the countenance wild; a hollow, stertorous sound issuing from the bottom of the throat; and yet suffering nature giving no vent to her feelings."—This may be termed the *passive* suppression of complaint; for when from false pride, or other motive, men have constrained by violence their feelings, the phenomena wear a different aspect. The face becomes swelled and reddened—the abdomen is flattened and contracted—the hypochondria project—the mouth is open, as if to give passage to cries which are stifled in the throat—the eyes are red, injected, but not moistened with tears.—The whole body bears the marks of violent efforts to overcome the pain. But even these expressions of the countenance can sometimes be suppressed.

"I extirpated," says Baron Larrey, "the breast of a lady affected with cancer, who was of a most religious turn of mind. She held a crucifix in her hand, smiling and talking to it in the most tranquil and placid manner, during the whole of the operation, while her body was contorted with agony. She was seized immediately afterwards with an universal spasmodic affection, which nearly put an end to her existence." "Another time," says the same distinguished surgeon, "I operated, for a large popliteal aneurism, on the master of a seminary who, though young, and apparently susceptible, bore the operation, which was very painful and complicated, without uttering a word, or moving a single muscle. But a terrible *re-action* succeeded this internal struggle between Nature and pain! Spasms and cramps assailed the suffering patient for the first fifteen days after the operation, and he did not recover under three months."

We trust that the observations contained in the foregoing

paper, may not be unprofitable to the surgical reader. Every thing which can mar in any degree the success of an operation is of great importance to the young surgeon; and we have repeatedly seen much mischief done by the almost universal habit which surgeons are in of repressing the language of complaint in their patients, and exhorting them to bear their sufferings without a murmur. This evil is increased by the *bravado* of the patient, who, in an operation room or public hospital, endeavours to stifle his groans, and act the hero, when it would be much wiser to shew himself the frail mortal, the child of pain, by giving a moderate and natural vent to the LANGUAGE OF COMPLAINT.

Observations on the Utility of Hedera Sylvestris (Ground-Ivy) in the Treatment of Mania. By E. SUTCLIFFE, Esq.

[From the London Medical and Physical Journal, for September, 1819.]

THE medical qualities of ground-ivy are not, I believe, sufficiently known to the medical profession, for it is not admitted into our Pharmacopœia. I have employed it for more than twenty years in cases of mental aberration, especially in melancholia, with the most advantageous results, and am disposed to think that it is a highly valuable remedy. It appears to act as a direct sedative. When its operation is salutary, it tranquillizes the patient; and when no organic disease is present, and the affection seems to depend on a casual cause of excitement, I am not often disappointed in my expectations of a favourable termination of the malady under its use.

The mode in which I have generally directed it to be employed, is a wine-glass full of the liquid juice twice or thrice a-day, but in some cases this cannot be exhibited; I have then ordered a proportionate quantity of the extract: but my observations have not led me to repose so full a degree of confidence in the latter as in the former preparation of the plant.

In cases of mania, where the high arterial excitement requires *local reduction*, I have found it, conjoined with abstraction of blood, generally to lead the disease to a favourable issue. In some

cases it has, however, not been productive of any benefit: in these, I suspect, there has been present some organic disease.

My object in making the preceding brief observations, is to draw the attention of other practitioners to this remedy, and to extend the benefits that may be derived from it beyond the circle of my own personal avocation.

Queen Street, Cheapside; July 8th, 1819.

From Observations on the Excretion of the Milk. (ŒUVRES DE BORDEU. Recherches Anatomiques sur les Glandes, § lxxiii.)

[From the London Medical and Physical Journal, for Aug. and Sept. 1819.]

IT is now universally understood, that the excretory ducts of the breast terminate in considerable numbers in the nipple, where they are replicated on each other in such a manner, that, if they are extended or made straight by drawing out the nipple, they permit the milk to pass with increased facility.

It is also known that an infant, in the first instance, merely elongates the nipple by drawing it towards him, and the milk thus flows into his mouth; besides that, he may also, by sucking, draw the milk of the nurse who suckles him: but this is a peculiar species of excretion, on which we shall not here dwell. It has some analogy with the effect of a cupping glass, and does not relate to my present object. Besides which, the mechanism of this is well explained in the *Memoires de l'Académie des Sciences*.

It should be remarked, that an infant, in sucking, besides extending the nipple, irritates or excites it; so that the breast itself enters into contraction, or into a sort of *erection*.

There is no nurse who has not experienced this tension, and a species of *titillation*, which is the consequence of it. They generally say, that they perceive the milk flow into the breast. The breast becomes round, firm, and distended; and there are some women who suffer drawing and tremulous sensations, which extend as far as the shoulders, the loins, and even into

the arms. These sensations are painful in some women; but they ordinarily experience a titillation more or less voluptuous.

These irritations have so much influence on the excretion of the milk, that there are many mothers who cannot suckle any other than their own infant, because they are not excited by others.

The infant has difficulty sometimes in managing every sort of nipple; and nurses find children who do not excite them sufficiently, who do not induce a flow of milk, or who do not cause those *titillations*, or those *tremulous motions*, of which we have just spoken: but there is hardly any one who does not find some infant that suits her, to which she attaches herself the more, because it repays her by exciting sensations to which tenderness succeeds.

One might believe, when an infant sucks, and when he touches the breast, handling it different ways, that he compresses it: the fact is, he elongates it a little, and excites it by friction.

There are some mothers who, when their infant touches them, are titillated to such a degree, that a sort of constriction in their breasts takes place, which prevents the flow of milk. There are some that are less sensible, who say that the contact of the infant produces an impression or modification of sensation in their breasts, which they are unable to describe, and which does not differ from that species of *erection* of which we have already spoken.

It must be acknowledged, that there are some nurses in whom the milk escapes on compressing the breast: it makes a jet, but this jet is not of long duration; it only arises from the evacuation of the largest of the lacteal vessels situate near the nipple; and if the breast does not enter into contraction, the excretion of milk does not continue.

There are also some nurses who lose their milk in this way at certain hours, after taking food: their breasts have passed through all the states of which we have spoken, and the vessels have been so full, that the milk has escaped from a sort of disgorgement; but this only takes place to a certain point, and but little also flows from compression.

It is necessary to observe what passes here with attention;

and, if we take care not to confound extension with compression of the breast, or with the changes which arrive in it by irritation, we shall be convinced that compression only causes the evacuation of part of the milk, which was contained in the largest ducts about the nipple; which are, as it were, little reservoirs, that they may be suddenly evacuated; but from which compression would never excite a continual flow of the fluid, without the causes which we have detailed.

We have seen women who have endeavoured to make their milk flow before the infant had sucked them, and put their breasts into action, but that has been impossible; but, as soon as the breasts had been thrown into contraction, by rubbing them and exciting the nipple, the milk has flowed of itself for a certain time, and could not be checked until the paroxysm subsided. This illustrates what we have stated above; and it is necessary to remark, that excitement of one breast is sometimes sufficient to put them both into action.

There are women who appear to have hardly any milk in their breasts, which are lax and empty; but, as soon as the infant excites them, they become distended, and the milk freely flows.

The history of the changes which take place in women in child-bed, and that of their maladies, demonstrates still better the species of contraction which we believe to be necessary in the breast, in order that the milk may flow from it. It is only necessary to pay attention to all these changes, to be convinced that the milk only appears in the breasts when they have been put into action; and it will be easy to perceive many phenomena which confirm it, that would occupy too much space in their detail.

Similar observations, which every person may readily confirm, and to which many additional peculiarities may be added, indicate the true cause of the excretion of the milk; which depends on a species of convulsive action, that, after having prepared the passages or canals terminating in the nipple, which has itself become tense, seizes the whole body of the breast, and disposes it to render its milk, when it is excited by the infant; which contributes, in its turn, to the excretion of that fluid by the suction it employs, as well as by the excitement of the organ which it produces.

[§ lxxiv.]

THOSE who have often witnessed the milking of cows, or who have had the curiosity to attempt effecting it themselves, must have perceived, that, as the peasants express it, *every body has not a good hand for it.*

Any rude compression of the teat will not do; it should be gently titillated and elongated; we then see the cow fix herself in her position, extend her thighs a little, and then the milk flows in abundance. If compression or extension of the teat were only necessary, every body could milk an animal.

It often happens that a person who knows very well how to milk cannot get milk from certain cows. Some are delicate in this respect, others are trickish: the former can only give their milk to certain persons; the latter will only do so to those that excite them in a certain manner. They commonly habituate themselves to those who have been accustomed to milk them; and it is often useless for strangers to attempt to effect it.

We sometimes see peasants menace, and even beat, their ewes, cows, and goats, until they choose, as they say, to give their milk; of which they are sometimes very avaricious. Some will only give it when they are amused by having something to eat set before them; others would never give it if their attention were thus distracted: there are in this respect many variations.

What happens with respect to their sucklings, also merits attention. It is known that they seize the teat, and that they elongate it, by pressing it in a sort of canal which they form with their tongue; they suck thus, and labour some time without being able to get any milk, which comes at length when the mother places herself in a certain posture: if she walks about, or if she amuses herself, the suckling does not get much; it is necessary that the mother be occupied by what regards her young one.

When the mother does not give her milk properly, what is it that the young one does? We see it assume a variety of motions; it shakes the teat, and presses it towards the contiguous parts: these agitations make the milk flow, not because

they compress the udder, but because they excite it, and put it into action.

A suckling which displeases its mother, as sometimes happens, or a stranger, that the mother will recognise by the rude efforts he makes, and the irritations he excites, will try in vain; he will not get a drop of milk; the mother preserves it for her favourite.

We have been assured by peasants, that there have been vicious cows, which would neither give any milk to their sucklings, nor to those persons who tried to milk them, and which nevertheless appeared to be well adapted for having milk. On watching them during the night, it has been seen that serpents came and hung by their teats, and sucked them; which the cows suffered with so much pleasure, that they showed their passion by continual lowings. It is a received opinion amongst the Pyrenees, that serpents have the talent of titillating the cows to such a degree, that, when they have been sucked by these animals, they will not suffer either their sucklings or the peasants to milk them.

There are women who assume an affection for certain children; there are also amongst brute animals some mothers who only take pleasure from certain sucklings: this is commonly their own, as we have already observed, or that to which they have been habituated. But some have been seen to form an attachment to young animals, which they would have fled from, or devoured, if they had not been retained by the delight of the titillation they have endured. Bitches, the most eager after game, have been known to suckle rabbits and squirrels, as well as young wild hogs and wolves, which certainly possessed their natural ferocity; since animals of the same species with the mother who nourished them, have fled from them with terror, or pursued them with rage.

There are some young animals, which, not knowing how to suck, disgust their mothers, whom the peasants are obliged to render favourable to them, by accustoming their teats to the sensation which the suckling, not having the talent to flatter the mother, excites; for the mother will only nourish it on the condition that she shall herself have her share of pleasure.

To these remarks we should add, that, with respect to

women, as well as to cows, goats, &c. those who have the largest mammæ are not those who give the most milk. They should be sensible in proportion as they are large, without which they become flabby or loaded with fat: they should also be disposed to enter into contraction, and to take on that action which elicits the milk and causes it to flow.

It must not be forgotten, that, if a mother be not for a certain length of time excited by her suckling, the breasts forget their proper function; they subside, and become indolent, and without action. There are some, however, which can recover it by simple suction; the breasts then awake from their state of drowsiness.

Precocious girls have sometimes been seen, who, having habitually suffered themselves to be sucked, have had milk formed in their breasts; but we know that the breasts commonly only dispose themselves to their functions in consequence of pregnancy. Although it may have happened that men have had milk, as it has been stated, and that this milk has been different from a species of serous fluid with which the breasts of young persons are more or less filled towards the age of puberty, we may dispense with the enquiry after the cause of this rare phenomenon.

Are not these reflections, then, which might be carried to a much greater length, as it would be difficult to explain all the phenomena relating to this subject; are they not sufficient to authorize the assertion, that the excretion of milk depends, at least in part, as we have already said, on the particular erection of the breasts themselves?

A Case of Hæmorrhage, which terminated fatally, from the Application of a Leech. Communicated by ANTHONY WHITE, M. B. Surgeon to the Asylum, Consulting Surgeon to the General Penitentiary, and Assistant Surgeon to the Westminster Hospital.

[From the London Medical Repository, for January, 1819.]

It has been rarely, if ever, recorded, that the application of a leech has been directly productive of death to the person on

whom it was placed: such an occurrence I have once witnessed; and on several occasions my assistance has been requested to restrain hæmorrhage which had continued many hours, and where every usual method had in vain been adopted, and where the blanched and exhausted condition of the patient was not sufficient to cause a spontaneous cessation of the flow of blood. The case of death to which I allude happened in my immediate neighbourhood: the circumstances were the following. A gland under the angle of the jaw became enlarged and painful, in a female child two years and a half old: the mother, at the request of her apothecary, placed a leech on the tumor; the direction given was, that when the leech had fallen off, a large poultice was to be applied to the bite, into which it might bleed; and it was requested that the poultice should be kept on all night. The leech was applied at night; the poultice also; the child put to bed: thither also, at the same time, went the mother.

The next morning the child was found with pallid face and bloodless lips, and its whole body bedewed with chilly sweat, the usual precursor of death, arising from slow hæmorrhagic exhaustion. The blood had flowed during seven hours, and was found in a large coagulated mass under the sheet. The unhappy mother had, of course, slept during this period: the volatile and other stimulants which I directed to be administered were of no avail; depletion had gone beyond the recoverable limit, and the patient died before I left the house.

It is remarkable that in this exhausted state blood was yet slowly oozing from the leech wound; and I have witnessed, in many other instances, where, even during fainting, the blood has flowed from the peculiar bite of this animal. Children, on account of many inflammatory affections, and where the use of the lancet is impracticable, become the frequent subjects of leeching; and many parts of the body, from structure and situation, are illy calculated to bear pressure, or to have it effectually applied. The relation of the following case, which led me to adopt a method of restraining hæmorrhage, is an example:—My friend, Mr. William Pritchett, had directed leeches to be applied over the trachea of a child affected with croup; the bleeding from one of the

punctures became alarming; styptics, after the usual fashion, had been in regular order applied, and all as regularly failed. On my seeing the child with Mr. Pritchett, I found it much exhausted, and the blood pouring over the throat in a rapid stream. The weak condition of the patient forbade me to reiterate the attempts which are usually adopted, and on closely inspecting the parts, a small pulsating arterial stream was discernible. On the spur of the moment the following method was adopted: I procured a fine sewing needle, which I passed through the leech-bite, piercing as much of the skin on each side as was sufficient to obtain a moderately firm hold: the bite or wound thus transfixed, I conveyed beneath the needle a few turns of common thread, which gave me complete and effectual pressure on the orifice. The method used by the farriers after bleeding the horse, which is to pass a pin through the orifice, and afterwards to wrap round or behind the pin a piece of hair or flax, nearly resembles the plan I adopted. A piece of cork or wax was adjusted on each end of the needle, which the next day was removed: the portion of skin necessarily pierced on each side of the wound is so small that very little pain is experienced by the operation. The plan is so simple, yet so certainly effectual, that I have for the last five or six years constantly adopted it, where, from situation or structure, pressure was inadmissible; or danger, during the night, of a recurrence of the bleeding was apprehended. In a very recent case in which leeches had been applied over the scrotum of a boy eight years old, with a congenital hernia, and where, arising from three of the punctures, hæmorrhage had returned during the night, the patient was with difficulty recovered from its excess, the needles were effectually applied. I have from this circumstance thought the subject of sufficient importance to the community to give publicity to the method which has invariably succeeded. The throat, scrotum, and perinæum, are parts of the body to which leeches are frequently applied, and which, from situation and structure, are ill adapted to receive or bear pressure: when the bleeding cannot be restrained with facility in these situations, I have oftentimes saved my patient much exhaustion

and alarm, and myself much trouble, by adopting the plan I have explained. Where the leech, with its teeth, has punctured a large cutaneous artery, which not unfrequently happens, and where, from its imperfect division, contraction cannot take place, I have occasionally succeeded in restraining the bleeding by pushing the point of a lancet into the leech puncture; but this method is uncertain of success. From the report of many of my professional friends it is certain, that infants occasionally perish from the effect of leech punctures; and many cases of great hazard, no doubt, frequently occur from the same cause to children beyond the infant age, and even to adults of delicate and lax structure. The circumstances connected with each case will regulate the adoption, among Practitioners, of the method which I have recommended. The histories I have detailed are of themselves, I trust, of sufficient importance to warrant this very simple, yet effectual, method of restraining hæmorrhage, being the subject of communication to the medical public.

Parliament Street.

SELECTED REVIEWS.

The Influence of Civic Life, Sedentary Habits, and Intellectual Refinement, on Human Health and Human Happiness; including an Estimate of the Balance of Enjoyment and Suffering in the different Gradations of Society. By JAMES JOHNSON, Esq., Surgeon to his Royal Highness the Duke of Clarence, author of the "Influence of Tropical Climates on European Constitutions," of a "Practical Treatise on Derangements of the Liver, Digestive Organs, and Nervous System," and editor of the "Medico-Chirurgical Journal." 8vo. pp. 93. London, 1818.

[From the London Medical Repository, for September, 1819.]

WE have read this little volume with a degree of attention equalled only by the gratification which its perusal has inspired. Alike amusing and instructive, it is written with all the intelligence and spirit that characterize the productions of its well-known author: and if Dr. Johnson were as successful in establishing the practice, as he has been in demonstrating the truth, of the important doctrines which it contains; if he could impress upon the public mind a deep and practical conviction of the advantages of dietetic moderation during health, and the value of abstinence in disease; we hesitate not to declare, that he would confer a more substantial benefit on his country than any writer of the present or preceding age; and perhaps contribute more largely to her real happiness than any of those warriors whose heroic achievements have shed such a bright though delusive splendor over her history, or the statesmen to whose genius and eloquence her Senates have been wont to bow.

But of consequences thus fortunate we utterly despair. Every one, conversant with the natural obliquities of the human mind, however it be gifted with original talent, or improved by culture and experience, will allow with us, that

such a revolution is, in times like these, more to be wished for than expected. Even within our own sphere of observation, every day brings with it some lamentable proof of the irresistible propensity to excess, although invariably followed by suffering and complaint; and that too in persons whose intellectual powers might raise them above the suspicion, as it should do above the influence, of this degrading spirit. If such then be the case with the more enlightened classes, what can we expect from those over whose sensual appetites neither reason, philosophy, nor the elevating sense of conscious dignity which they usually inspire, exerts its salutary restraint; and who find in the gratification of their brute pleasures, their only solace or recreation from a life of care and labour. Indeed it is but too true, that to restrain the appetites and passions of the multitude, even in their most obviously destructive tendencies, is an attempt equally vain and desperate as that of arresting time in its progress, or repressing the fury of the ocean when agitated by the storm.

Hopeless, however, as is the prospect of any general revolution in the habits of the country, with respect to dietetic indulgences, much, we are convinced, might be effected by the steady and consistent exertions of the medical Profession, towards diminishing the enormous mass of wretchedness and disease of which they are productive: and in this view, we really consider both ourselves and the public deeply indebted to Dr. Johnson, for the pleasing and forcible manner in which he has illustrated the value of moderation and the consequences of excess; and in his estimate of the enjoyments which they yield, he has invariably shown the balance to be in favour of temperance and simplicity.

Still be it recollected by professional men, that precept, however eloquent, is, unless sustained and illustrated by example, utterly unproductive. If we wish to inculcate temperance on those around us, we should study to acquire and to maintain a character for temperance ourselves. In vain may we declaim on the importance of simple and abstinent habits, if we show not by our own conduct that we have sufficient confidence in the result to resist the temptation of their infringement. With equal grace and effect may the physician

declaim on the virtues of a frugal meal, while gorging his own ill-fated stomach with all the heterogeneous poisons of the turtle feast; or prescribe water with one hand, while with the other he pours down copious libations of alcohol to the jolly god.

It may even be contended that medical men are, to a certain extent, responsible for the habits of the district in which they practise and reside. The sins of omission, though less flagrant in their aspect, are frequently almost as pernicious in their consequences as those of commission: and the practitioner who does not his utmost to resist the encroachments of drunkenness and gluttony on the well-being of those around him, is surely little less guilty than the man who, by his open and unblushing example, contributes to propagate the dire contagion. May not a spirit of intemperance, assuming a decided prevalence in any particular tract of country, frequently admit of explanation on this principle? Poets may point their epigrams, or philosophers vent their bitterness of disappointed pride, at the expense of the physician; the ignorant, in the hey-day of health, deride his character and resources; but the hour commonly arrives, when the flashes of wit, the tone of supercilious arrogance, and the pointless attempts of vulgar sarcasm, are exchanged for the more humiliating language of complaint, of confidence, and implicit submission to his dictates: and then what a strong and lasting influence over the opinions and conduct of his patient the once despised man of art is frequently enabled to acquire, they alone who are conversant with scenes of human suffering and depression can adequately comprehend. Nor are the extensive purposes to which this influence may be subsequently converted more valuable in the hands of conscientious and consistent practitioners, than pernicious in those of the profligate and unthinking. The celebrated Dr. Darwin is said, by his doctrines and example, to have effected a complete revolution in the previously intemperate habits of the gentlemen of a whole county.

These are, perhaps, ungracious and unpalatable truths: but experience has convinced us of their correctness; and we deem it a duty to record them. It is now, however, high time to

return from this awkward and perchance unprofitable digression, and retrace our steps to the path from which we almost unconsciously deviated.

The work of Dr. Johnson consists of a preface; an introduction, on the impulse of civic associations; and three chapters, with their corresponding divisions, wherein the influence of civic life, &c. on *the digestive organs*, through the medium of *food, drink, air, sedentary habits, mental emotions, late hours, and medicine*; on *the heart and circulating vessels*, through the medium of the *digestive organs, skin, and passions*; on the *lungs and glandular system*; on the *animal or muscular system*; and on the *brain and nervous system*, through the medium of the *digestive organs, heart, and passions*; is respectively traced and developed with a very strong and masterly hand. The whole is terminated by an estimate of the balance of enjoyment and suffering in respect to the intellectual system.

Into a laboured analysis of the substance of this interesting volume, we do not propose to enter; for, in the first place, its excellence consists, not so much in any striking originality of fact or opinion, as in the soundness of the physiological and pathological views disclosed in it, and the apt and familiar and perspicuous manner in which they are illustrated: and, secondly, we are unwilling to obviate, by the exhibition of a sketch which, after all, can afford but a very imperfect notion of its value, the necessity every one of our readers, anxious to keep pace with the progress of the science, will doubtless feel, of cultivating an acquaintance with the original. Indeed, as the production of an active, highly intelligent, and experienced observer, it contains views and observations, an ignorance of which would be alike disgraceful and detrimental to the practitioner; and, as clearly exposing the more common agents whereby the beautiful regularity of function in the human economy, essential to health and happiness, is troubled or destroyed, and the leading principles upon which such disorders may be rectified or prevented, it possesses no feeble claim to the attention of those enlightened men who study medicine; and so all who aspire to that character should study it,

as a branch of liberal education—an important link in the vast chain of universal science.

The following extract, the only one which our limits allow us to transcribe, will sufficiently indicate the correct and comprehensive views entertained by Dr. Johnson, upon the subject of diseases and their remedies. We select it the more willingly, because we feel, and wish to bear testimony to the justice of the reproof which it conveys. Not even Mr. Bailey himself, of Belladonna celebrity,* relied with more implicit confidence upon the unfailing virtues of his deadly narcotic, in the treatment of neuralgia, than we, in unriper years, on our favourite combination of opium and calomel. Experience has, however, shown us the fallacy and danger of such sweeping conclusions. And we are anxious to expiate our error in the only way in which expiation is practicable—by its manly and public avowal:—

“ It has been a just cause of reproach to this country in particular, that we are fonder of studying remedies than indications; that is, that we hunt too much for specifics, and do not sufficiently attend to the minute features, phenomena, and causes of disease, by a knowledge of which we might more effectually employ those remedies we already possess. Let us exemplify this observation. A lady is seized with that painful affection, *tic douloureux*, or face-ach. One person recommends calomel and opium, as an effectual remedy; a second proposes Fowler’s solution; a third asserts, that belladonna is a specific; a fourth, that cutting the nerve is the surest remedy. Now, any *one* of these may happen to be the right remedy; but they may *all* be wrong, and the poor lady may run the gauntlet before she is cured. Thus, if the face-ach be merely symptomatic of some derangement in the liver or digestive organs, the calomel and opium will probably be successful: if the disease arise from a translation of gouty or rheumatic irritation to the part, Fowler’s solution may stop the paroxysms of pain: if it be, as it seems, a purely nervous affection, belladonna may remove it: and if it consist in an inflamed state of

* See “*Observations relative to the Use of Belladonna,*” &c. By John Bailey. 1818.

the neurilema, or covering of the nerve, the division of that covering, by the knife, may so empty the vessels as to check the disease: but if, as is often the case, the sentient extremity of a nervous twig be irritated by a carious tooth, the whole of the foregoing means will be useless, and the extraction of the cause alone will destroy the effect."—Pages 39, 40.

With respect to the value of thoracic percussion and abdominal pressure, as applicable to the diagnosis of organic lesions of the heart, Dr. Johnson seems to assume a tone far more confident and sanguine than our own somewhat extensive experience in their employment, and that of our professional friends, have hitherto been found to justify. It were needless to observe how greatly the character and utility of any instrument are eventually injured by an extravagant appreciation of its worth; and hence how important to determine, as precisely as possible, the real advantages which may be derived from it. This we propose, ere long, formally to attempt with regard to the two processes in question; and shall now pause only to correct an historical error respecting their original introduction, into which Dr. Johnson has been inadvertently betrayed; Corvisart did not "first recommend" percussion of the chest and abdominal pressure to the notice of the medical world. The former of these processes was, many years since, communicated to the public, by Awenbrugger, a Physician of Vienna, in a Latin work expressly written for the purpose; and subsequently rescued, by the celebrated Frenchman, from the oblivion into which it had nearly fallen: while the latter had its origin with the profound and lamented Bichat; to whom it appears to have been suggested by his observations of the distressing effects of a distended stomach, upon subjects labouring under organic affections of the thoracic viscera.*

There is yet another, and, in our estimation, much more valuable diagnostic sign of a certain class of diseases of the heart, to which we are surprised that Dr. Johnson has not even adverted; since it is scarcely possible that a phenomenon,

* See Mémoire sur l'Application comparée de la Percussion de la Poitrine, et de la Pression Abdominale. *Œuvres Chirurgicales* de P. J. Dessault. Par X. Bichat. Tome III. page 340.

so common and so conspicuously marked as this, can have escaped his penetrating eye. We allude to a peculiar undulatory pulsation, or rather alternate turgescence and collapse of the external jugular veins. It is most commonly seen in those cases wherein great irregularity of the heart's functions is complicated with enlargement of the liver, and peritoneal and general dropy consequent on the obstructed circulation in the venous system of the latter organ. Whenever this phenomenon has clearly and permanently presented itself to our observation, the distressing prognosis, founded upon it, has never once been eventually belied. The hepatic and dropsical complications may, indeed, be removed for a time by mercury and diuretics; but the heart scarcely participates in the relief thus obtained; and the external jugulars retain their irregular thrilling action. In a short time the abdominal affections fail not to recur with aggravated and often ungovernable violence. The patient sinks; and on dissection, we believe, will be commonly found contraction of the auriculo-ventricular opening of the right side of the heart, and dilatation of the corresponding auricle; thus elucidating, almost with the clearness of demonstration, the source of the phenomenon exhibited by the jugular veins, and the cause of the obstruction of the liver, with its concomitant effusion of serum.*

Again; in a science which rests, like that of medicine, upon the accumulated observation and experience of ages, we feel ourselves utterly at a loss to comprehend how any writer can assert, as our author has done in his preface, that he is totally unindebted to other men's ideas in the construction of his work. Such an avowal is not less ungracious than substantially incorrect, since we all are daily, however unconsciously, availing ourselves of the labours of others; and hence unworthy of the liberal and elevated character of *Dr. James Johnson*. For ourselves, while with the very instrument of correction in our hands, we are proud to acknowledge the valuable hints and sound instruction which we have, on many occasions, derived from his able and impressive writings.

One more observation, and we have done. That the beauty

* We are not aware that this appearance has been noted by any author.

and effect of Dr. Johnson's fine and even fascinating style, should be so frequently slurred by the injudicious admixture with stale, or, perhaps, sometimes irrelevant quotations from the poets, we, in common with many of his best friends, have long in silence lamented. Would that any thing we could say might open his eyes to the flagrant injustice of which he is thus guilty towards himself; and, to employ his own figurative language, convince him of the impolicy of the attempt to entwine the wreaths of poetry around the severe and unbending brow of science! We sincerely revere his talents. We feel an interest in their effectual application, more deep and anxious than he will perhaps be inclined to credit; and would willingly incur his temporary displeasure, if, by so doing, we might promote his lasting welfare, and conciliate his lasting esteem. Conscious we are, that when time shall have somewhat tempered the enthusiasm of his feelings, he will look back with an eye of regret upon the errors to which we have adverted, and acknowledge, with us, that the language of the muses is not more inapplicable to medicine, than that of indiscriminate eulogy to the duties of sterling friendship. Meanwhile, we shall, indeed, feel gratified if the admonition or applause, here uttered, should have the effect of removing any obstruction, or cheering him onward in his ascent to that proud elevation of rank and character, which evidently constitutes the goal of his unceasing labours and aspirations; and which, we confidently predict, he is, ere long, destined to attain.

An Inquiry illustrating the Nature of Tuberculated Accretions of Serous Membranes, and the Origin of Tubercles and Tumours in different Textures of the Body. With engravings.
By JOHN BARON, M. D. London. Longman and Co., 1819, pp. 307.

[From the London Medical Repository, for August, 1819.]

IN that part of the "general review" which occupied a large portion of our preceding number, we took some notice of this volume. We acknowledged the ingenuity with which the author had treated the subject; we expressed our pleasure to

find him unshackled by those fashionable theories which refer every thing pathological to vascular impetus and inflammatory action; but we remarked that Dr. Baron's particular views regarding the origin of tubercle and tumour required more time and attention than we could then spare. Since those observations went to press, we have taken some pains to examine the foundations of those pathological views which this author supports. The importance of the subject, the mass of information which he has collected together, the acknowledged talents of the author, and the extent of experience which his situation, as Physician to the General Infirmary at Gloucester, has given him, all entitle them to a candid examination; and if we differ from him, and even ultimately form a less favourable opinion of his work than our first impressions might have led our readers to expect, we trust he will believe that we are still very sensible of the value of his observations, and thoroughly satisfied that the object of the learned and benevolent author is to "advance the cause of knowledge, and mitigate the sufferings of humanity."

This volume is only the commencement of a work, whose extent is not very easily to be defined; for no one, by reading the first chapter, could possibly imagine that it was to branch out in the manner which it does towards the end. It is divided into two parts, the first of which comprises remarks on the tuberculated accretions of the peritonæum, and gives us a glimpse of the author's views regarding the connexion between tubercle and hydatid. The second part treats of the tuberculated accretions of the pleura, and unfolds the full extent of those speculations on the origin and connexions of tumour, tubercle, and hydatid, which it is the chief object of this book to promulgate. As the opinions of the author are, to say the least of them, very singular, we naturally expected to find them brought together in a sort of tangible shape, towards the conclusion of the volume; but we were grievously disappointed to arrive at the last page without any such summing up, which the author says "it is perhaps wise to abstain from for the present." We say we were much disappointed at this, because it put us under the obligation of rading the book through once more, in order to lay them before our readers;

setting aside the risk, that in making such an attempt ourselves, we may not be dealing very fairly with the author. We flatter ourselves, however, that he will not have much occasion to find fault with us, when we say, that the following seem to be the principal conclusions to which he comes:—

1. That there is a disease of the peritonæum and other serous membranes, characterized by the formation of tuberculated accretions, which, though frequently mentioned by authors as occurring, has never yet been regularly inquired into, so as to establish its pathological connexions.

2. That this disease of serous membranes, and several others with which it is pathologically associated, have hitherto been considered as originating in inflammation; but that this opinion is gratuitous and erroneous.

3. That the diseases with which this peculiar tuberculous affection of serous membranes, and especially of the peritonæum, are connected, are tubercles in the lungs, and tubercles and tumours in various other parts of the body, cancer, fungus hæmatodes, “the whole tribe of cachectic diseases,” and possibly some diseases of bone.

4. That tuberculated accretions of serous membranes and these various kinds of tubercles and tumours are closely connected, in a pathological point of view, with the formation of hydatids: that hydatids are the sources of all these morbid structures, and that they are susceptible of transformation “from the simple watery vesicle up to the schirrous and cartilaginous texture.”

5. That this question is totally independent of the origin of hydatids themselves; but that the notion of their being animals is by no means satisfactorily made out.

6. That the diseases now referred to “belong to an important family, whose origin may be more satisfactorily ascribed to the absorbent than to the sanguiferous system.”

Such are the leading points of Dr. Baron’s speculations; and we must confess, that however desirous we are to see the minds of our pathological writers divested of the prejudice which makes inflammatory action necessary to every change of organic structure, we are yet startled at the boldness of some of the conclusions which we have now detailed, and which

the author does not announce with any degree of diffidence, except indeed on one occasion, only, as far as we can remember*; but considers as fully proved; referring to his "well supported train of reasoning," and to authors who "caught glimpses of the right track," and "advanced near to the discovery of the truth, without actually penetrating the veil."

When an author uses these and similar expressions, it is to be presumed that he has cleared the subject from the mists of prejudice, and brought forward such arguments as must carry conviction to all. We are afraid, however, that there are still many who will refuse assent to these doctrines, and who, if they even acknowledge the insufficiency of former theories, will hesitate ere they adopt the explanation which the author of the present treatise wishes to substitute. It is time, however, that Dr. Baron should speak for himself; and we think we can introduce all that is really important in the book to the notice of our readers, by making a few observations on each of the six leading points of doctrine which we have stated it to be his object to inculcate.

In the first chapter of the work, Dr. Baron gives a most excellent detail of a number of cases of that tuberculous disease of the peritonæum which appears to have been the first to attract his notice. Many of these occurred in his own practice, and some are transcribed from the works of former authors. The frequency of this disease is probably much greater than is generally imagined. In our own practice we have seen a very considerable number of cases of it: and the author of this volume, by directing the attention of the profession in so particular a manner to this very singular affection, has conferred on us a very essential benefit. We are rather surprised, however, that in this division of the work no mention was made of Dr. Pemberton's valuable treatise on the diseases of the abdominal viscera, in which this affection is very admirably described, under the title of chronic inflammation of the peritonæum. Dr. Pemberton, it is true, does not make any distinction between simple adhesion and the granular or tuberculated disease of the membrane; nor does he even mention

“those fleshly and vascular appendiculæ or tubercles being suspended, like grapes, from its surface, into the cavity of the abdomen,” which constitute the peculiarity in the morbid appearances presented by the disease under consideration; but he distinctly describes the insidious origin, the slow progress, the symptoms, diagnosis, and prognosis of the complaint, and the principal other appearances found upon dissection; particularly the agglutination of the bowels into one mass, the great thickness of the peritonæum, and the occasional throwing out of a dropsical fluid. In the chapter on diagnosis*, no attempt is made to establish a distinction between the chronic peritonitis of Dr. Pemberton, which we have always been led, from the day we first entered on the profession, to consider a disease as strongly marked as any in the whole nosology, and the tuberculous disease of the membrane. We are at a loss, therefore, to know whether the author was aware of Dr. Pemberton’s accurate description of such an affection, and whether he considers it the same, or different from that of which he treats.

For our own parts, we are inclined to think that there is a tuberculous disease of the peritonæal membrane, different from common chronic peritonitis; but we confess our inability, in the present state of pathological knowledge, to determine, with any degree of certainty, their diagnostic symptoms. Before any such attempt, however, is made, it will of course be necessary to *establish* the position which we have only ventured to suggest as a matter deserving of inquiry.

2. Dr. Baron imagines that chronic inflammation is incapable of explaining the phenomena of this affection of the peritonæum. His words are, “Nothing that is known of the physiology or pathology of the sanguiferous system seems capable of explaining what we are now in quest of†.” Against the general doctrine of chronic inflammation he sets his face in every part of the book. We scarcely recollect that the phrase is ever used. Now upon this point it is right that we should come to some understanding with the author. As we said in our last number, we are perfectly ready to admit that we

* Chap. iv. page 38.

† Page 85.

“daily found cysts, containing fluids and substances of various kinds, imbedded in the viscera, or attached to them, or appearing on the surface of the body, when no perceptible inflammatory symptoms existed;” that “inflammation is not always a cause of altered structure; but that it many times is a consequence rather than cause of that state;” that “tubercles in the lungs exist anterior to inflammatory action; and scrofula, in its commonest forms, shows itself in lymphatic obstructions and tumefactions, long before other changes take place:” but we do not exactly see how all this is applicable to a particular disease of the peritonæal membrane; still less, that chronic inflammation of vessels is altogether to be kept out of view, in attempting an explanation of its appearances. There is a curious passage from the writings of Bichat quoted by Dr. Baron, page 74, wherein that ingenious author notices the peculiarity of this disease of serous membranes, the “productions d’une foule de petits tubercules blanchâtres qu’il est si fréquent de trouver sur ces membranes.” At one time he speaks of it as a chronic inflammation; at another he maintains that it should be arranged in some other class than that of the phlegmasiæ, and that it should be considered as characterized by the formation of little tubercles, and an affection peculiar to those membranes, as miliary eruptions to the skin, or aphthæ to mucous surfaces. The same difficulties that struck Bichat, and which betrayed him into this inconsistency, appear to us still to involve the question, nor have the few arguments which Dr. Baron brings against the theory of chronic inflammation, consisting chiefly of one or two extracts from Dr. C. Smyth’s paper on the varieties of inflammation, been able to solve them to our satisfaction. Dr. B. indeed adds two cases which appear to him to prove “the occurrence of inflammation with all its well-marked symptoms and consequences, after the change of structure peculiar to the former disorder, the tuberculous disease, had existed.” These cases are certainly in point, but they do not affect the leading obstacle to the establishment of the new theory, viz: whether there are any cases which from their commencement deserve the name which Dr. Pemberton, we believe, first gave them, of chronic peritonitis. We conceive there are, and that Dr. Baron has erred

in not drawing any distinction between them and the cases of pure tuberculous disease. We cannot, therefore, subscribe to the position which he has advanced, at least in the extended sense in which he wishes it should be taken; "that chronic inflammation cannot be assumed to illustrate the origin and progress of a great variety of the most fatal and alarming chronic diseases, without involving the reasoner in the most palpable contradictions and absurdities*."

In page 121, we read that "disappointments in the treatment of the tuberculated disease of the peritonæum first led the author to distrust the doctrine which gave inflammation for its cause;" and in page 122, "that he soon abandoned the idea of chronic inflammation." Now, without offering an opinion as to the pathological question which this sentence involves, we would wish to ask, Whether there is not something illogical in this style of reasoning, and whether it would not equally apply to any other theoretical speculations on a disease, which the author is candid enough to acknowledge "no remedies in our present state of knowledge can subdue, and which generally leads to a fatal termination†?" Dr. Baron, in support of his theory, brings forward John Hunter's observations on collections of matter which are formed without inflammation. But were we disposed to prosecute the subject further, we would ask, Whether, in such cases, there may not take place what Dr. Baillie has conjectured to occur in some diseases of the trachea, a peculiar action of vessels analogous to, but differing from, inflammation‡?

We shall presently find that Dr. Baron, when disappointed with the inflammatory action of blood-vessels, seeks a substitute in a very different quarter.

3. The author tells us that his views gradually enlarged during the progress of his inquiry, and assumed at length a comprehensive aspect, the extent and bearings of which, if seen at first, might have deterred him from entering on so wide a field of discussion. In fact, it is curious to observe how the subject "grew upon him." From contemplating an obscure affection of the peritonæum he began to associate it with simi-

* Page 117.

† Page 18.

‡ Baillie's *Morbid Anatomy*, page 99, 125.

lar diseases of other serous membranes. He extended his views to tuberculous formations in all textures of the body, particularly to tubercles in the lungs. His next step seems to have been to unite them with the different varieties of phthisis. Cancer, fungus hæmatodes, fatty tumors, and some diseases of bones, are next brought within their influence, until at length he thought he could trace the connexion between the tuberculated disease of the peritonæum, from which he set out, with "the whole tribe of cachectic diseases, and a vast number of disorganizations which occur under various modifications in every texture of the body."

Now there is something in this sweeping system of generalization which does not accord with our ideas of the intricacies and difficulties of the science of pathology, of the slow march of general science, or of the extent of original observation, likely to fall within the scope of any single inquirer. To establish upon a sure basis the pathological doctrines connected with any one of these morbid derangements of structure, would ensure lasting fame to an author; and we confess we should have looked with a less jealous eye upon the foundations of the reasonings contained in this volume, if we had observed less tendency to make them applicable to such various modifications of disease. We are quite satisfied, that in the science of medicine the truth does not lie so near the surface as such speculations would lead us to suppose. We have only to look back to the style of medical reasonings, as presented to us in the best authors of the last century; nay, we have only to cast a very superficial glance at the *present* state of medical science, to convince ourselves, that the steps in the progress of its improvement are made very slowly, and that all attempts to grasp, by a premature effort, the labours and triumphs of posterity, have been, and will probably continue to be, unavailing.

That there is a disease of the pleura and of the arachnoid membrane, and of the capsular ligaments of joints, very similar to that of the peritonæum which Dr. Baron has so fully detailed, we have no doubt; and it will be for him to point out the connexions of these diseases, and the analogies that prevail in their origin, progress, and symptoms. By so doing,

we may safely venture to predict that he will be conferring a benefit on science; but we would suggest, that this should be done before any other link is added to the chain; and, at any rate, we must take leave to enter our protest against any train of reasoning, which would, without the amplest proof, associate together diseases, which, whether rightly or wrongly, we have for centuries back been in the habit of considering as fundamentally different.

These remarks we wish to apply particularly to the case of tubercles of the lungs. Authors have agreed, from very early times, in acknowledging the obscurity which hangs around the origin of these bodies; and we have been long desirous that this subject, opening so important a field of investigation, should be prosecuted by some one who will devote that time to it, and give it that degree of consideration from which we might hope to derive advantage. We have little hesitation in saying, that such a result can be looked for only from such means, and that the inquiry must begin, and perhaps will end in the particular structure in which such appearances are met with. The hopelessness of any attempt to establish an affinity between tubercles of the lungs and the great variety of morbid changes of structure, which we have stated to be comprehended within the author's views, must, we think, be obvious to every one who is at all conversant with morbid anatomy.

4. But the greatest novelty in Dr. Baron's speculations is his attempt to point out the connexion of tuberculated accretion, of tumour, and tubercle, with hydatids, and to show that *they* are, in fact, the real origin of all the modifications of structure which we have detailed, and susceptible of transformations hitherto, we believe, unthought of; that is to say, from the simple watery vesicle, up to the schirrous and cartilaginous texture. To confirm this point, all his reasonings tend; and we must do the author the justice to say, that he has laboured it with great ingenuity; and if his facts and observations do not fully bear him out in all his views, yet still that he has brought forward much valuable information, which may have great weight in the estimation of some, and which will afford useful reflection to all. The principal argument in favour of his idea is, the circumstance of hydatids being so frequently

found in combination with tuberculated accretion. He mentions, page 110, one very striking case, where tuberculated adhesions were met with on both sides of the pleura, tubercles in the mesentery, the pancreas enlarged with tuberculated adhesions to the neighbouring parts, and where hydatids were found in and upon the liver, and attached to the external surface of the right ventricle of the heart, and a large cyst existed in the posterior mediastinum. The great difficulty, however, is still to be got over:—In what manner is the transformation of the hydatid from its state of vesicle to that of solid tubercle effected, and how is this transformation proved? Upon this point Dr. Baron is very explicit. He tells, that “such transformations were referred to by Boerhaave, conjectured to occur by John Hunter, and actually proved to occur by Dr. Jenner.” This last fact not being generally known, it may be worth while to state the grounds on which it rests. Dr. Jenner “demonstrated the position by experiments instituted on some very young animals, particularly rabbits. He found, that by feeding them with some kinds of food, the liver soon became studded with hydatids; and by examinations, at different times, he was able to trace the gradations already mentioned, from the first inspissation of their contents, and thickening of their coats, to their final conversion into tubercles of varying size and hardness. In the same liver he has also repeatedly found every gradation, from compact caseous looking matter, to the limpid contents in their first stage.”

Some of the facts connected with this subject were applied by Dr. Jenner to explain the origin of tubercular phthisis. He sent two communications to Dr. Beddoes on this subject, which are to be found in his work on factitious airs; and if the observations on which they are founded are correct, they certainly, as Dr. Baron says, have not yet met with the consideration they merit. Considering how long ago these remarks were published, it does appear surprising that they were never before applied to the explanation of the pathology of tumour and tubercle. We have not had time to make the necessary inquiries into a part of medical science, in which we confess our education to have been very much neglected; but we cannot refrain from the suspicion, that there must be some mistake

either in the observations, or in the reasonings built upon them; or so remarkable a circumstance would not have escaped the notice of Dr. Baillie, whose opinion, by the way, in these matters, we are disposed to rely very much upon, and who, in the last edition of his valuable work, makes no mention whatever of any such conversion of the hydatid vesicle. This, it must be borne in mind, is but a suspicion. If Dr. Jenner be right, the profession is indebted to the author of this treatise for recalling to its recollection an important and neglected fact. Until these experiments and observations of Dr. Jenner are satisfactorily shown to be incorrect, we certainly think the weight of evidence is on our author's side.

On the 5th point which we stated Dr. Baron to have urged, we entirely agree with him. It seems to us quite indifferent, in relation to this question, whether hydatids be, or be not animalcules. The same conversion of them into tubercles and tumours may take place, whatever be their origin and real nature. The author certainly inclines to the idea, that hydatids have not an independent existence, from his expression, page 276, "admitting, for the sake of argument, that they are animalcules;" But Dr. Baillie's remarks* on this subject appear to us to be very strong; and though fully satisfied "that the origin of hydatids is a question of less importance than the consideration of the vast variety of formidable changes of structure to which they give birth†," we think it would have been advisable in the author to have stated his reasons for doubting the commonly received opinions of pathologists on this point.

6. Dr. Baron makes no secret of his anxiety that the absorbent system should take place of the sanguiferous in explaining the origin of tuberculated accretion, tubercle, tumour, and hydatid; but he evidently fights shy of the question in the present volume. The following passages, however, sufficiently point out the tendency of his speculations.

"Disorders of the absorbent system are supposed to occasion many of the symptoms of a numerous, and most unman-

* *Morbid Anatomy*, page 241.

† *Inquiry*, page 111.

ageable class of diseases. To this class may, perhaps, be referred the disease under consideration*.”

“It is a very old opinion, that hydatids arose from the lymphatic system. Many strong analogical facts have been mentioned in confirmation of that opinion, and there is good reason for believing that it will ultimately be proved by direct observation†.”

The formation of large tumours by lymphatic vessels appears to us to be a theory involving many very great difficulties, and some obvious inconsistencies: but while we say this, we think it but reasonable to suspend our judgment on this point, which the author by no means presses, until from the forthcoming volume, which is to contain observations on diseases of mucous surfaces, and some other affections of serous membranes, we receive some “further illustrations of the subjects discussed in this inquiry.”

We have now gone over, in a very hasty manner, the principal novelties of this volume. There are one or two points, however, which escaped our notice in this review, which we must be permitted to recur to for a moment. By the term *tubercle* Dr. Baron denotes disorganizations composed of one cyst, whatever be its magnitude, or the nature of its contents. By that of *tumour* he understands those that appear to be composed of one or more *tubercles*. All tubercles are originally hydatids; and the process of transmutation of the vesicle into complex structures, or what Dr. Baron calls the arrangement of the elementary parts of morbid growths‡, is not left to conjecture, but is illustrated by a series of observations and diagrams. These are not so satisfactory to us as to the author; but we have no time to dilate further on what seem to be the weak points in the author's theory.

The supposition that hydatids are the original sources of *cancerous* structure, was long ago entertained. Sir Everard Home distinctly states, that he was led, from every consideration he could give the subject, to believe that such hydatids are no part of the poisonous disease; but where found in combination with it, are to be considered as accidental complaints

* Page 86.

† Page 301, note.

‡ Page 218.

superadded to the cancerous disease*. Dr. Adams, in his "Observations on the Cancerous Breast," takes a different view of the subject, and maintains that hydatids are essentially connected with the origin of cancer. We were a good deal surprised at Dr. Baron's severe animadversions on these views of Dr. Adams. He maintains, it is true, the animalcular nature of hydatids; but we are at a loss to see why this opinion is "the fundamental error of his book," and why it is calculated to involve the subject, and render what he has brought forward little available to the purposes of true science," when Dr. Baron himself confesses, in the same page, that "the question, so far as pathology is concerned, has nothing to do with the speculations respecting the origin and vitality of hydatids†." This, considering how very nearly Dr. Adams's notions accord with his own, appears to us to show a want of liberality on the part of the author, which we should not have expected, after the manly avowal in his preface, that "these coincidences are very valuable," inasmuch as "they vouch for the accuracy of different observers, and prove that their minds have not been warped by the influence of preconceived opinions."

We are not among those who maintain, that to constitute a valuable book in medicine, it is necessary that it should contain some new or improved system of treatment. We do not, therefore, consider the merit of this volume at all diminished, because the chapter on treatment is barren of novelties. The only peculiarity that struck us was the recommendation of long continued nausea and vomiting in discussing tubercles, and tumours, and scirrhus, and even in combatting the advanced stages of pulmonary consumption. Dr. Jenner, whose observations on this subject are quoted with approbation, seems to imagine that all our commonest deobstruent medicines act on this principle, digitalis, squills, nitre, white vitriol, &c.; and that the little success which frequently attends their use is perhaps to be ascribed to the too limited mode of their exhibition—to the interval of freedom from nausea being too long. Some curious cases, illustrating the effect of long continued nausea in the removal of morbid growths, are detailed.

* Home on Cancer, p. 108.

† Inquiry, p. 277.

Having, in the early part of this article, collected into a focus the principles which the author believes he has established in the course of his inquiry, upon most of which we have taken the liberty to animadvert, it may not be altogether uninteresting to conclude by recapitulating those facts really made out, or, in our opinion, rendered probable, by Dr. Baron. We think, then, that he has fairly made out,

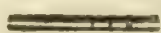
1. That there is a tubercular disease of serous membranes, which has not hitherto been examined in a comprehensive point of view; that it runs a defined course, and that its causes and treatment are involved in much obscurity.

2. That the doctrine of chronic inflammation of vessels is not adapted to explain this and several other morbid modifications of structure, and that it requires further investigation.

3. That hydatids are susceptible of conversion into structures totally different from that which they originally enjoyed; and that they may have their origin in some state of vessels analogous to that which gives rise to the tuberculous disease of serous membranes.

4. That the origin and pathological relations of tumours is a subject upon which much light may still be thrown by accurate observation and cautious deduction.

Further than this, we are not disposed to go: but even these are great steps in the progress of medical science: and we have again to request the author will accept our thanks for the instruction which his very interesting and ingenious volume has afforded us.



A Memoir on Inflammation of the Veins. By G. BRESCHET,
Prosector to the Faculty of Medicine of Paris, &c.

[From the London Medical and Physical Journal, for August, 1819.]

THERE are but few diseases that have been less understood, until within a late period, than that which forms the subject of this memoir. Observations respecting it may be found in the works of Aretæus, Paré, Dionis, Platner, Boerhaave,

Van Swieten, and Morgagni; but the nature of the affection was not determined until it was demonstrated by John Hunter. Since then, various instances of its occurrence have been published, a general reference to which was given by us on a former occasion, when considering the paper on this subject by Mr. Carmichael in the Transaction of the Association of the Dublin College of Physicians. The cases above alluded to being widely dispersed in medical literature, many of them have hitherto been unknown to the generality of surgeons; though, from the nature of the disease, it is of the highest importance that every one should possess all the information that can be furnished respecting it by historical records. To collect those cases, to arrange them in appropriate order, and to deduce from them some general conclusions, has been the object of M. Breschet in the memoir under consideration; and it is with much pleasure that we are enabled to offer to our readers the results of the labours of this intelligent and experienced surgeon on so interesting a subject.

We shall, in passing through this memoir, give a reference to the observations which may be supposed to be generally known to English surgeons; an abstract of those which have not hitherto been published in our language, and of those proper to M. Breschet; and conclude with the pathological reflections of this surgeon, and his remarks on the mode of treatment best adapted for their relief.

§ I. *Inflammation of veins caused by phlebotomy.*—In this section, Dr. Breschet adduces the cases related by Hunter, Sherwin, Mr. Abernethy, Mr. Charles Bell, and Mr. Travers; and details three instances of it which occurred to his own observation. The first patient, a man 22 years of age, who, having been bled in the arm several times for epilepsy, suffered after the last pain about the place of the incision; which was followed the next day by redness, tension, and swelling, extending in a few days from the shoulder to a little below the elbow. The pulse was weak and frequent; fever came on, with prostration of strength; very severe pain in the left side of the chest, with difficulty of respiration; which, after a few days, terminated in death. Examination of the parts then showed the cephalic vein to be full of pus

throughout its whole extent. The superficial radial vein contained also pus, until about two inches from its origin.

A woman, aged 20 years, was, on the 20th of July, bled in the left foot, for a vomiting of blood attributed to derangement in menstruation. She soon began to suffer very severe pain about the place of the incision, the edges of which became hard and everted. Swelling gradually extended to the superior part of the limb, accompanied with pain and tumefaction of the inguinal glands, fever, and restlessness. These accidents continued until the 23d, when they began to subside under the use of fomentations, cataplasms, rest, and laxative medicines. The saphena vein was then discovered, hard, tense, and increased in bulk, from about two fingers'-breadth below the ankle to the height of the origin of the tendon of Achilles. Pressure directed towards the opening, not yet cicatrized, produced an evacuation of a small quantity of thick pus, streaked with red. On the 26th, the pains and the suppuration had ceased, and the wound had healed. The vein, although diminished in diameter, still preserved its hardness, when the patient left the hospital on the 5th of August.

A man, 34 years of age, was bled three times within fifteen days from the right arm, for vague pains in the limbs of a year's standing: he was soon afterwards bled also in the left arm. On this occasion, symptoms of inflammation of the median cephalic vein ensued. "This case terminated favourably. On the sixteenth day from the accident, the vein had the appearance of a small cord, much less in diameter than the vein of the opposite side, evidently not admitting of the transmission of blood."

§ II. *On inflammation of veins produced by the ligature of them, and by the excision of varices.*—"We know," says Mr. Breschet, "that Hippocrates punctured varices; Ætius, Paulus Ægineta, Avicenna, and Albucasis, have described a method for the excision of them; that this operation appears to have been practised by Fallopius and Severinus; that the two Fabricii comprised the varicous portion between two ligatures, and emptied it by incision; and that Ambrose Paré, Dionis, Petit, and others, opened varices to extract the coagula from them, and afterwards approximated the sides of the

vessels by compression. Nevertheless, a great number of facts attest, that the ligature and the excision of varices may induce serious accidents, of which death has been a frequent consequence."

M. Breschet then notices the results of the operation of tying the saphena major, practised a few years ago in St. George's Hospital; and the results of a similar measure related by Mr. Travers in his *Surgical Essays*.

M. Breschet has not noticed the important improvement in the mode of performing this operation, effected by Mr. Brodie, which has been attended with such advantageous results in his practice, and in that of Mr. Carmichael.

§ III. *Of inflammation of the veins after amputation.*—An abstract of the observations of Hunter and Mr. Travers, constitutes this section.

§ IV. *On inflammation of the veins consequent on the ligature of the umbilical cord.*—"Meckel," says M. Breschet, "has witnessed in an infant, a short time after its birth, vomitings, colics, diarrhœa, icterus, fever, and nervous affections: death took place on the tenth day. The peritoneum was inflamed and covered with a false membrane, and there was effusion of pus into its cavity. The branches of the vena porta, and especially those of the umbilical vein, were very tumid, and their parietes much thickened; those of the umbilical vein, and its first branches in the liver, were of the thickness of one or two lines, and were covered with a firmly-adherent spurious membrane. Osiander has published an analogous fact: an infant was attacked with erysipelas, that soon spread all over the body; and it died on the third day from its birth. The intestines and the liver showed signs of inflammation on their external surfaces; they were covered with false membranes and purulent matter. The umbilical vein, from the navel to the liver, contained pus of a yellow colour. Meckel has also furnished us with the history of an infant that was born with a strangulated inguinal hernia, consequent on compression of the abdomen during its passage from the uterus. The hernia could not be reduced before the third day: severe colics remained, accompanied with tympanitic distension of the abdomen, icterus, and vomitings. Death took place on the seventh

day. On opening the body, all the disorganization consequent on peritonitis was discovered: the parietes of the umbilical vein were much inflamed, and covered with pus; and there were spots of ulceration on its internal surface."

The analogous observations published by Dr. Colles,* of Dublin, who has witnessed *trismus* as a frequent attendant on this disease, have escaped the notice of M. Breschet.

§ V. *On inflammation of the crural veins after parturition.*—Several cases of this affection have been published by M. ckel.† "A woman died a few days after parturition. A short time after her delivery she was attacked with fever, and darting pains in the cavity of the abdomen and in the pelvis, which after a time disappeared: but, at the end of about three weeks, uneasiness in the region of the liver and in the left hip, and intolerable pain in the left thigh, supervened. On examining the dead body, the abdominal cavity was found filled with purulent matter; the liver was very voluminous, the lungs healthy. The crural vessels and nerves were surrounded with puriform matter. The crural vein, from its origin to the knee, was of the thickness and the firmness of the artery: it was filled with pus and blood. Its tunics cracked under the scissars; its internal membrane, well washed, was more spongy than in the ordinary state, and covered with a very distinct false membrane, which might be detached from it in slips. Its valves were in part corroded, ragged; and in part thickened, tumified, and of a deep-red colour."

The analogous observations of Mr. Travers, Mr. Wilson, and Dr. Clarke, are next cited; and this section concludes with a remark, that "M. Chaussier has frequently seen, in women who died after parturition, the abdominal veins full of sanious pus; and M. Ribes has observed, in a woman who died of peritonitis, at the same epoch, the same veins gorged with purulent sanies."

We may here remark, that, as no mention is made of the state of the vessels themselves in the latter cases, it is not sa-

* Dublin Hospital reports, vol. i. See London Medical and Physical Journal, vol. xxxix., for an abstract of these observations.

† Bibliotheque Medicale, tome xvi.

tisfactorily shown that they were instances of *phlebitis*; the "sanious pus" may have been taken up by them from extraneous sources.

§ VI. *On inflammation of the veins of the uterus and the ovaries after abortion.*—"A woman," says M. Breschet, "having aborted in the seventh month of gestation, the placenta being necessarily extracted by art, was attacked with fever, a miliary eruption, and pains in the loins and in the pelvis. Purulent matter flowed from the genital organs and from the rectum. After death, there was found, between the posterior paries of the uterus and the vagina, an abscess which opened into the rectum. The veins of the uterus and the ovaries were inflamed, their parietes thickened, their cavities ulcerated and filled with pus. The whole of the right renal vein, and the part of the vena cava that receives it, were inflamed, red, tumefied, and filled with a coagulum of blood, which in its middle contained some puriform matter."*

§ VII. *On inflammation of the veins by their direct communication, or their contact with other diseased structures.*—M. Fizeau has published a case† of suppuration in the hepatic veins, connected with organic affection of the biliary canals; in which M. Breschet is disposed to attribute the inflammation producing the former affection to the latter cause. M. Breschet next adverts to the observations on this subject in the essay of Mr. Travers, and then relates a case that occurred to M. Patissier: "In a subject who died of tetanus, the internal membrane of the auricles and ventricles of the heart, especially of the right side, were found of an intense red colour, chiefly at the basis of the mitral and tricuspid valves: the internal membrane of the aorta, the common trunks of the carotids, the subclavian arteries, the inferior vena cava, the jugular veins, and the pulmonary artery, presented red appearances that diminished in proportion as they diverged from the heart, which were not removed either by repeated washing, or even on scraping the membrane with a scalpel."

M. Ribes has often met with inflammation of the veins in erysipelas. He says, that, if this disease induces suppuration,

* *Bibliothèque Medicale*, tome xvi.

† *Ibid.* tome xxxviii.

which is very rare, the parietes of the veins are red, especially the internal membrane; their thickness is also increased, and their cavity filled with pus. In the case of gangrene, they are black, easily torn, and contain sanious matter. If the gangrene has not proceeded to the last stage, the parietes of the veins are a little dilated and very thick; their internal membrane, at the circumference and in the neighbourhood of the focus of the disease, is red, and visibly inflamed: they contain pus or sanies. M. Ribes has sometimes met with this sanies to the extent of several inches from the focus of the malady.*

§ VIII. *On inflammation of veins produced by metastasis.*—

A boy, 14 or 15 years of age, was affected with an eruption that had been considered to be the itch, which soon disappeared on the use of some ointment. He was almost immediately attacked with violent and continued pain in the right lower extremity, which became the seat of serous effusion. When he entered the hospital, he had all the symptoms of what is commonly termed nervous continued fever: he died after about three weeks. The vena cava was blackish in colour in some parts, whitish in others, was easily torn, and contained a white medullary matter, not coherent, which increased in quantity as it extended towards the iliac trunks, before the bifurcation of which the vessel was dilated and completely obstructed by the matter just described. The trunk of the left iliac veins was also obstructed by it; and the branches of it, although much dilated, entirely empty. In place of the iliac trunk of the right side, there was a sort of ligamentous canal, with thick parietes, and extremely small calibre, which terminated in a vast abscess formed in the cellular tissue surrounding the hypogastric and iliac vessels. Notwithstanding the most careful research, not the least vestige of the right crural vein could be found: its course was occupied by a train of pus, which extended to the ham. The veins of the right leg were contracted, and filled with solidified fibrine.

§ IX. *On inflammation of veins from chemical or mechanical causes, the presence of worms, &c.*—Treutter, Fabricius,

* *Memoires de la Societe Medicale d'Emulation*, tome viii.

and Rudolphi, describe worms which have been found in veins. Pus absorbed from ulcers of a malignant character, hospital gangrene, &c. some cases of caries of the bones, have been attended with inflammation of the veins. Sasse produced it in dogs, by injecting solution of opium, and tincture of euphorbium and cantharides, either into the jugular veins or into the cavity of the abdomen.

We have now to adduce the general conclusions of M. Breschet on the pathology of inflammation of the veins, and the appropriate measures for the relief of that disease. As these consist of observations rather than opinions, and are drawn up in a concise and perspicuous manner, we shall give them in detail, and without any particular remarks, as the accuracy of the author's descriptions, and the utility of his labours, must be sufficiently apparent. Dr. Breschet observes, that

“ Inflammation of the veins may be either idiopathic, sympathetic, symptomatic or metastatic.

“ *Physiological characters of inflammation of the veins.*—The signs of inflammation of the veins are not always easily recognized; particularly if the inflammation is seated in the small vessels, or even in the trunks, when they are situated in the visceral cavities.

“ 1°. *Local characters.*—The consideration of the precursory circumstances; pain in the course of the veins; swelling of the adjacent cellular tissue; sometimes redness and tension of the skin; its sensibility and shining appearance in the passage of the vein; and afterwards a knotty, tense, and painful cord, in the particular direction of the vessel, are the most ordinary phenomena of this inflammation. If a wound exists, there may be an evacuation of blood altered from its usual appearance, or of pus more or less thick in consistence, from the orifice of the vein. In the latter case, soon after the lesion of the vessel was effected, there is pain in the wound; swelling comes on; the wound does not unite by the first intention, its edges become hard; or, if it has cicatrized, an abscess forms under the cicatrix, and soon destroys its continuity. The pains which occur in the course of the inflamed vessel are more particularly directed towards the heart than towards the extre-

mities; they are augmented by pressure, and they have sometimes been supposed to be rheumatic. A sensation of burning heat follows exactly the same direction as the pain.

“ 2°. *General Characters.*—It is rare that inflammation of a vein, especially if it has existed some time, is not accompanied with disturbance of the system, or a real febrile state, the intensity of which varies according to that of the inflammation, the extent of the disease, its seat, the importance of the vessel, and the tendency of the local affection to one or other mode of termination. It should be remarked, that many physicians have observed in these circumstances the phenomena of typhus; and I have myself found, in many subjects who had fallen victims to typhus, evident traces of inflammation in the encephalic veins and in the venous sinuses of the cranium. The duration of inflammation of veins is occasionally very short; most frequently, it is of rather long continuance: and it appears, from the observations of M. Fizeau, that this phlegmasia may be remittent, presenting actual paroxysms of exacerbations of the symptoms.

“ Inflammation of the veins may be confounded with that of the arteries, the lymphatic vessels, or of the nerves. In the first case, the pain and the other external characters are directed from the point where the artery has been injured towards the branches of the vessel. In the second case, the pain may extend in the direction towards the heart, but the lymphatic glands swell, become painful, and the skin often presents two or three red waving lines, which appear shortly after the lesion or pricking of the vessel; and it is not long before tumefaction of the whole member takes place.

“ Lastly, if the nerve be the seat of the malady, the pain is propagated from the situation of its origin to a common centre, and especially from the situation of its ramifications: it is instantaneous; it immediately follows the puncture; and this pain has varieties of character, as we observe in the neuralgiæ, which are often only chronic inflammations of the nerves.

“ *Anatomical characters of inflammation of the veins.*—Inflammation of the veins may terminate in several ways. We do not possess well-authenticated facts to prove that it, in any cases, terminates in resolution; and that the vessel, being re-

stored to its former state, executes the functions for which it was primitively adapted. Union of its parietes, obliteration of its cavity, suppuration, ulceration, and gangrene, most commonly take place. Thus, phlebitis appears under the following forms: 1°, adhesive inflammation; 2°, suppurative inflammation; 3°, ulcerative inflammation; 4°, gangrenous inflammation; 5°, eliminatory inflammation.

“1°. *Adhesive inflammation of the veins.*—John Hunter and M. Dupuytren think that adhesive inflammation may exist in veins; and it is indeed by that, that wounds of these vessels are healed in four-and-twenty or six-and-thirty hours. We must, however, distinguish the cicatrization of the external tunic of veins from that of their internal membrane: the former, when it is divided, unites in the same manner as wounds of the cellular tissue; but this is not the case with the adherence of the internal membrane: it takes place more remotely and with more difficulty, from inflammation less readily affecting this tissue. The cicatrix of solutions of continuity of the parietes of veins remains but weak for some time, and is easily torn. Twelve or twenty-four hours after blood-letting, we can, by slight percussion or by distension, destroy the union that has been effected. It is even by these that the adhesive inflammation is changed into the suppurative, and is propagated to a greater or less extent in the cavity of the veins, and sometimes to the heart.

“John Hunter, however, considered the adhesive inflammation of veins as a mean of arresting the progress of the inflammation; and he says, that, by compression excited on the sides of the veins, we may cause them to adhere, and limit the extent of the disease, by preventing the pus formed below the adherence extending further towards the heart. Mr. Travers believes, on the contrary, that this adhesive inflammation is rare in veins, that a considerable degree of irritation is necessary to provoke it, and that, when inflammation is developed, it passes rapidly to suppuration; or else, that a large quantity of coagulable lymph is effused into the vessel, which fills up its calibre, obliterates it, and at length becomes adherent to its parietes: but here there is, properly speaking,

only adhesive inflammation, only that there should not be so abundant an effusion of coagulable lymph.

“ The little disposition to adhesive inflammation, offered by the internal membrane of veins, seems to oppose the analogy that has been supposed to exist between that and the serous tissue.

“ 2°. *Suppurative inflammation.*—It is very common in veins: the greater part of the observations that we have referred to, demonstrate its existence. This species of inflammation has a great disposition to extend further and further on the affected surfaces, and the pus mingled with the blood in its circulation, with the continuity of the tissues, is a powerful agent in its propagation. It is in these circumstances that the general phenomena assume their severity of character, and that the state of the patient becomes calculated to excite the most serious alarm. If the inflammation diminish, the pus, from ichorous, serous, or white and consistent, as it had been, is changed for an albuminous fluid, and secondary adherence or obliteration of the vessel is the consequence.

“ 3°. *Ulcerative inflammation.*—There are many authentic instances recorded of ulcerative inflammation of the veins. M. Portal mentions having found, in the body of a woman, an ulceration of the superior vena cava. Mr. Travers cites a case, where the internal jugular vein was obliterated by the pressure of a tumour situate on the right side of the trachea, and covering the large vessels. The patient, towards the termination of his existence, had voided blood mixed with pus, by the mouth and by the anus. On dissection of the parts after death, the tumour was found to contain gangrenous cellular membrane and blood in a state of putrefaction. The internal jugular vein was filled, to a certain extent, with a coagulum of blood; but below this there was an ulceration that had established a communication with the sac of the tumour, so that the blood which came from the head passed in part into the cyst; there was an ulceration also that had induced a communication of the cyst with the œsophagus, so that the contents of the tumour could pass into the intestinal canal.

“ It has been frequently observed, that, if wounds of the veins have not united by the first intention, the suppuration

that is formed, either in the tunics of the vessel or in the adjacent tissues, was soon accompanied with inflammation of the edges of the wound; that the opening gradually increased in size; and that it was not rare to see the whole circumference of the vessel destroyed by this vessel, and the continuity of its canal cease to exist, by the progress of the ulceration. Lastly, many instances of ulceration of the internal membrane of veins have been observed, but without the perforation of the whole thickness of the parietes of the vessels.

“4°. *Gangrenous inflammation.*—The frequency of suppuration of veins leads to a belief that gangrene might readily affect these vessels: observation, however, demonstrates the contrary. The veins and arteries long resist the effects of the gangrene which destroys all the tissues in the midst of which they may be situated. These vessels are, however, not in a healthy state; inflammation has seized them, and an abundant quantity of albuminous matter, mingled with a little blood, fills them, obliterates the canal for a greater or less extent, and thus opposes the occurrence of hæmorrhage. At length, the vessels, isolated on all sides by the destruction of the other tissues, and no longer receiving nourishment, die by a sort of inanition; a process is set up in some point, but always below the termination of the coagulum; the part of the vessel despoiled of its adjacent parts is separated; and this inflammatory process, by which parts deprived of life are separated from others, is that which I term, after M. Dupuytren, the *eliminary inflammation*.

“5°. *The eliminary inflammation.*—When a ligature has been placed on a vein or an artery, it is by this eliminary inflammation that the vessel is separated throughout the whole thickness of the line embraced by the ligature, by which this is also detached, and by which a portion of the vessel forms a real eschar. In an artery, if the ligature be not too large and is properly applied, a moderate degree of inflammation affects the vessel above and below it. A slight effusion of coagulable lymph takes place on its internal membrane, and adhesion by the first intention is the result; a coagulum of a conical figure, albuminous at its base and sanguineous at its summit, is found, near the adherence of the

sides of the vessel, to oppose the effort of the blood; and, by eliminatory inflammation, the artery is gradually destroyed in the point correspondent to the ligature, which at length is detached. This happens from the tenth to the fifteenth day, if the vessel alone has been comprised in the fold of the thread, whatever may be its nature; but this is longer in taking place, if the thread does not act immediately on the vessel, and if it comprehends fibrous parts. The coagulum is not adherent to the internal membrane in the first few days; but, by an inflammation that takes place in the tissues of the vessel, it terminates by forming an adhesion.

“If any cause opposes itself to the union of the vascular parietes in the points adjacent to the ligature, and when the coagulum is not yet adherent, hæmorrhage may occur; and, if a ligature of reserve be employed, we easily divide the whole of the parietes of the artery in a point where the coagulum does not occupy the whole calibre of the vessel, and where it consequently cannot adhere to the internal membrane. This section of the arterial parietes by a ligature of reserve, is effected more promptly in proportion as the inflammation has made the tissues of the artery lose their resistance, and permit themselves to be divided like parts affected with the sebaceous degenerescence. These observations on the mode of resistance of inflamed tissues, and on the true causes of consecutive hæmorrhages, appertain to M. Dupuytren. I have often had occasion to verify their justness and importance.

“If an hæmorrhage happens a short time after a ligature has been placed on an artery, it is not immediately above the first that a second should be applied, but on a distant point, where the arterial parietes are not inflamed. Has a large ligature been applied on an artery? The section of the vascular parietes has not been effected, and union by the first intention is not effected; it is the suppurative and ulcerative inflammation that then ensues; and, when the whole thickness of the vessel has been destroyed by this process, hæmorrhage is the consequence.

“It is not exactly thus with respect to the veins. Adhesive inflammation being excited with more difficulty in these vessels, and their internal membrane not offering the same faci-

lity of division as that of arteries, adhesion by the first intention does not take place, or only with great difficulty. The facility with which the tissues of veins are distended with blood, is another unfavourable circumstance to the primitive union. A more powerful and a more durable irritation is necessary to induce inflammation of veins. Albuminous matter alone is secreted if the inflammation be moderate, or else to this secretion is joined the formation of pus if the inflammation be intense. When the inflammation extends on one of the surfaces of the vessel, either from the force of the irritation that has acted on it, or because the pus has circulated in the part of the vein which is between the ligature and the heart; it is, without doubt, to this propagation of the inflammation, that the development of general symptoms, the severity of the disease, and often the death of the subject, is to be attributed.

“The part of the vein situate near the ligature inflames; the membranes of the vessel acquire an inordinate thickness; the canal of the vein is filled up, and to a great extent, with a concrete albuminous matter, which soon adheres to the vascular parietes, and renders the vein impervious to the blood. The ulcerative and eliminatory inflammation then divide the whole thickness of the vessel in the circular line embraced by the ligature.

“*The treatment.*—It should be directed by the same principles as that of other inflammations. We should first endeavour to ascertain its cause; and, if it has happened after venesection, determine whether or not the instrument has been broken, and the point of it left in the parietes of the vessel.

“When the inflammation is solely local, we have been advised to put a stop to it suddenly, in its commencement, by cold lotions or fomentations,—as ice and saturnine applications. When the disease is more advanced, it should be combatted by the application of leeches on the course of the affected vessel; relaxing fomentations; emollient cataplasms; oily and mucilaginous unguents; tepid baths; camphorated, opiate, and narcotic, topics.

“J. Hunter, Reil, and Mr. Abernethy, think that we

should, and that we may, incite adhesive inflammation above and below the part affected, to oppose the extension of the disease, and prevent its extending further and further up the venous trunks, and to the heart itself. This adhesion would also prevent the pus, circulating with the blood, from irritating the internal membrane of the vessel over which it might flow. From what I previously said, it will appear that we cannot depend on the effects of compression in exciting this adhesion, because it is very difficult to obtain adhesive inflammation in the veins.

“A more certain mean would be to cut the affected vein through transversely, at a certain distance from the seat of the disease, to break the continuity of the tissues, and thus oppose the propagation of the inflammation. We may conceive that this operation is practicable only in cases where the affected vein is not very large in calibre, readily accessible to instruments, and when we need not fear the wounding of an artery or an important nerve.

“Prudence requires that, in the operation of phlebotomy, we always endeavour to obtain union by the first intention of all the different parts interested; and, if new emissions of blood are desired a short time after the first, that we do not destroy the process of primitive cicatrization to obtain blood by the same opening, but that we make another in a second part, and, if it be possible, in a different vein.

“If the inflammation has terminated by suppuration, we should, without delay, open the abscess to obtain a free course for the evacuation of the purulent matter.

“Lastly, if general symptoms appear, and they are intense, we should combat them by all the means directed for the treatment of angeio-tenique (inflammatory) fever and the phlegmasiæ.”—*Journal comp. du Dict. des Sciences Medicales*, tome iii.

Practical Observations on the Action of Morbid Sympathies, as included in the Pathology of certain Diseases; in a Series of Letters to his Son, on his leaving the University of Edinburgh, in the Year 1809. By ANDREW WILSON, M. D. Kelso. One vol. octavo, pp. 407. London, 1818.

[From the Medico-Chirurgical Journal, for January, 1819.]

IN various parts of this Journal, but particularly in our review of Dr. Parry's Elements of Pathology, we took occasion to protest against the present rage for tracing diseases almost exclusively to *vascular* derangement, and overlooking the morbid movements or state of the *nervous* system. The longer we live, the more we see, and the deeper we study, so much the more are we convinced that, not only are the *primary impressions* of morbid causes sustained by the *sentient* system of the human fabric, but that it is in *this* system the primary morbid *movements* first begin, and are thence propagated to the *vascular* system; which, from that moment, reacts upon, and is again influenced by, the nervous system. We never shall have a sound pathology or successful practice, while any one of these two systems is studied in preference to the other; or, while they are considered as unlinked in perpetual reciprocity of action. Under this conviction, we shall give every possible publicity to the labours of those observers of Nature, who are exploring this interesting subject; and we could not have a better opportunity than in the analysis of Dr. Wilson's volume—a work which is characterized by deep thought, solid judgment, and accurate observation.

Dr. W. justly observes, that the laws of morbid sympathies are often very demonstrable to the senses, “and may be reasoned on in diagnostics with the utmost propriety, and considered as steady principles to be acted upon.” Page 7.

“We know, for instance, that certain irritating matter contained in the stomach, will very commonly produce a headache; but knowing also the universal influence which the stomach exercises over the whole human frame, if a person becomes affected with pain, &c. in a distant part of the body,

in a limb for instance, without any occasional cause having been applied, attended at the same time, with certain signs of a loaded stomach, reasoning from the above facts, we readily believe, that if irritation in the stomach can produce pain in the forehead, it may, at any other time, produce pain in any other distant part also. It is a fair enough inference, therefore, that in this case likewise, the primary disease is seated in the stomach, and is a cause of pain in the limb by an act of morbid sympathy." Page 8.

The brain and its appendages, indeed, being the medium through which the living principle acts upon the otherwise inert organic structure, as also the medium through which the mind is made conscious of injurious applications offered to any part of the machine, it may properly be said that every part of the human body is capable of sympathising with the rest; because we see that no part of it can admit of having the sense of feeling excited to the height of pain, without the whole frame being deranged in sensibility. There are, however, specific sympathies, which are so common in their occurrence, as to be noticed as things of course; as, for instance, the gastro-cerebral, the cutaneo-gastric, and the utero-gastric sympathies.

These morbid sympathetic impressions on distant organs or parts are sometimes attended with pain, and sometimes not. In the *latter* case, the original irritation only increases or decreases the excitement in the sympathising structure. Thus we see the *vascular* system sometimes thrown into *inordinate* action by acrimonious irritation in the alimentary canal; at other times, the action of the heart is almost suspended by the application of offensive substances to the nerves of the stomach. But when the sympathetic irritation arises to the degree of *spasm*, which, in fact, is only the highest degree of contractile action in a muscular or fibrous structure, then we have *pain*.

"Wherever, therefore, muscular fibres exist, morbid spasm may also exist; consequently, not only the fibres of large muscles, but the smallest fibrous threads of all the membranous parts, are also susceptible of spasm from irritation, applied either directly or indirectly from morbid sympathy.

"It is further to be observed, that although the most violent

spasms of large muscles are seldom, if ever, succeeded by local inflammation, as is instanced in epilepsy, hysteria, &c.; yet, when the muscular texture of a membrane becomes the seat of a sympathetic spasm, *local inflammation of that membrane is speedily induced, the coats of its capillary vessels being probably also soon included in the sympathetic influence.*" P. 22.

This is an important doctrine, which is ever to be borne in mind by those who separate spasm from inflammation, and never dream of the *latter* when they have to combat the former.

With regard to the vinculum, as Dr. W. justly observes, connecting the sympathising part with that where the irritation is applied, we remain totally in the dark; and therefore, all we have in our power on this point is, by careful observation, to mark demonstrable facts; and these, by candid induction, ought to lead to correct practice.

The extensive influence which a healthy or diseased state of the digestive organs, and of the stomach in particular, exercises over the whole human frame, mental and corporeal, has long been, and still continues to be, the subject of observation and wonder among pathologists. When, indeed, we consider that the mucous membrane, stretching from the mouth to the anus, and lining so many important hollow viscera, is a secreting surface, studded with innumerable nervous or sentient papillæ, and exposed daily to a mass of heterogeneous and stimulating matters, as well what is poured in, in the shape of food and drink, as poured forth from the mucous glands themselves, in the shape of morbid secretion, it is not to be wondered at that the primæ viæ are the seat of most of those primary irritations which are productive of disease. The morbid sympathies which subsist between the stomach and various distant parts are doubtless reciprocal; but those which radiate *from* the gastric centre, would appear to be more powerful than those which are propagated from distant parts to it. It must also be remembered, as was hinted before, that irritation in the fibrous structures of the membranes lining the hollow viscera, and entering into the composition of joints, &c. will induce, first, spasmodic action; and whenever *this* arrives at a certain height, inflammation in the structures under conside-

ration. This local inflammatory movement will soon draw the heart and vascular system into increased action, by morbid sympathy—and thus a febrile state is induced. The following passage from Dr. Wilson depicts precisely our own sentiments, and accords with our own practice and experience.

“In fact, the operation of the *laws of sympathy*, through the nervous influence on the living system, is a thing demonstrable to the senses; and any theories which I have ventured to offer on their pathology, are very nearly as much so, to an unbiassed mind. At least, this I will assert for myself; that to my conception, they approach far nearer to the certainty of demonstration, than any other pathology I have met with on the same subjects, and have served to *direct my own practice*, in a satisfactory manner through a mist of insulated facts and general assertions; while every additional year’s experience has served more and more to strengthen my opinions.” P. 36.

We believe that Darwin, the great father of the sympathetic doctrines of modern times, was the most *successful* practitioner that England or the world ever produced. There was scarcely a town on the Continent of Europe from whence he was not consulted, either personally or by letter; and we have it from good authority, that his success was mainly owing to the *multiplied resources* which the sympathetic views of disease opened out to him. In our own practice, for many years past, we have had *solid* reasons for pursuing the doctrines and practices of Darwin; and we seriously recommend them to the rising generation, who are not yet so blinded by prejudice or pride, as to be beyond the reception of any thing in the shape of advice from others.

In the fourth Letter, our author enters on the consideration of febrile diseases. He considers that “a permanently increased action of the heart and arteries” is a *sine qua non* in fever, no other phenomenon being invariably present. We think, however, that a *deranged action* of the heart and arteries is a more permanent symptom in fever than an *increased* action; but we shall not cavil about definitions. It is of more consequence to bear in mind that this increased [deranged] action of the heart and arteries may arise

“From irritation, applied to the nerves of some distant part

of the body, between which and the vascular system a primary affinity subsists, exciting that system into violent action by morbid sympathy; or from the effects of something of an irritating nature having been received into, and circulating with the blood, and thereby directly applied to the internal surface of the heart and blood-vessels themselves. But the establishment of a febrile state of the system, as derived from remote causes within the body, appears very demonstrably to acknowledge the following primary distinctions:

“1st. As derived from the effects of acrid matter applied to some department of the nervous system, and exciting a morbid action of the heart and arteries by morbid sympathy; but such a remote cause, experience shows, is most commonly seated in the digestive organ.

“2d. As derived from the effects of irritation, arising from organic obstruction, somewhere existing in the body, and exciting increased action of the heart and arteries, by morbid sympathy.

“3d. As derived from the effects of something of an irritating nature received into, and circulated with the blood.

“4th. As derived from passions of the mind, exciting an increased action of the vascular system, by morbid sympathy.

“5th. As derived from two or more of these primary causes of fever acting at the same time.” P. 43.

Our author candidly acknowledges, that we are totally ignorant why gastric irritation should in one person excite the vascular system, and in another the voluntary muscular system; but we are certain that such is the fact. Dr. W. then proceeds to the consideration of gastric fever, as the most common, and at the same time, the most simple form of fever. He considers the irritation in the stomach as propagated, by morbid sympathy, to the heart and arteries. In this species of fever, the first signs of any departure from health are of that kind which point out impaired action and morbid irritation in the digestive organs, as is evinced by the precursory phenomena of diminished appetite; weight at the præcordia; abdominal distension; thirst; parched mouth, and furred tongue. To these are commonly added, signs of impaired and irregular action in the nervous system, arising from morbid sympathies with

the primæ viæ; such as languor, disturbed sleep, head-ache, dry skin, flushings and chills. This is the prevailing fever of infantile years; and, if taken in time, may soon be checked, merely by the ejection of the source of irritation from the primæ viæ; another proof of the truth of the doctrine in question.

The two great indications then in gastric fever are, the evacuation of the morbid secretions from the line of the alimentary canal, and the reduction of morbid temperature on the surface. Dr. W. here takes occasion to object to the unreasonable prejudice which has lately existed against emetics in this country, chiefly engendered, we conceive, by their deleterious effects in the hotter regions of the earth, where the gastric and biliary systems are ordinarily in a state of irritability, which is greatly exasperated in the fevers of those climates, and of course where emetics are improper. But in this country it is very different, and physicians lose a powerful febrifuge by the disuse of emetic tartar in particular.

“Nor are antimonial emetics attended by any hazard, nor even by more severe nausea than ipecacuan, unless it happens by an over-dose, or by mismanagement during its operation. One grain of ant. tart. for an adult, will generally nauseate the stomach in half an hour; when, without waiting longer, the stomach should be distended by drinking warm water, and vomiting is promoted by irritating the fauces a little with a feather. One half-glass of wine after every fit of vomiting will assist its operation.” “It is by a prudent and free administration, then, of antimonial emetics, along with mercurial laxatives, assisted by the neutral salts, that the first and great indication of cure is here to be accomplished.” Page 60.

The Doctor takes this opportunity of expressing his opinion, that the *debility* in these fevers is only apparent, not real; and of course, to be removed or prevented by evacuations, which take off the load from the spring of life. He justly remarks, that mere *aperients* will not do; but that “very free evacuations are always necessary, procured by the help of the most active *purgatives*, administered in appropriate doses; remarking the nature of what comes off, till it puts on a healthy ap-

pearance, a circumstance which usually attends a solution of the fever, with its accompanying train of symptoms." P. 62.

Our author next turns to the consideration of fever, as dependent on "idiopathic organic obstruction;" viz. topical inflammations, from whatever occasional source they may be derived, as local stimuli, wounds, bruises, &c.

"The application of excessive cold, or of burning heat, &c. forming obstructions in the capillary vessels of the vascular system, and exhibiting what is designed local inflammation; from whence, by a sympathetic affinity, the heart and arteries are drawn into violent action, and it thereby becomes a primary cause of fever." P. 71.

After some ingenious remarks on the nature of the topical inflammation itself, in which he concludes that the obstruction to the free stream of blood is seated in the capillary extremities of the veins, our author observes, that with respect to the *treatment* of fever in these cases,—

"Large and general bleedings, as also topical bleeding, whenever it can be applied, are perhaps the most powerful means; and along with these, aperient medicines. But, whenever an acting morbid cause is observed to exist in the digestive organs, a free discharge from these, *by the most active evacuants*, is a measure of great importance, from the mutual sympathy betwixt the stomach and every part of the body. As to external applications, I have always observed tepid fomentations to be the most useful, and the most soothing to the patient's feelings." P. 80.

The ratio symptomatum and pathology of typhus are next descanted on by our author, with much ingenuity and acuteness; and although we cannot agree with him on all points here, particularly in his opinion that the gastric and biliary secretions are greatly *increased* at the beginning of typhus, yet his doctrines all tend to support the propriety of the depletory or anti-stimulant plan of treatment, now superseding the Brunonian mania. Dr. W. believes, and we unite our suffrage to his, that cinchona stops the paroxysms of an ague, not through the medium of the circulation, but by its tonic, and perhaps sedative effects on the nerves of the stomach. Indeed, it is only

in this way that the salutary operation of several other remedies in intermittent fever can be accounted for.

In respect to our author's *methodus medendi*, we think it is not quite energetic enough; though, probably, in the part where he resides, and where fevers are not likely to assume the graver types, the practice may be good enough.

Dr. Wilson's long chain of reasoning on influenza we must pass over, in order to pay some attention to his opinions on acute rheumatism. Here the sceptics of the day will be ready to say, that Dr. W. far outstrips our illustrious London pathologist, Mr. Abernethy, in attributing a variety of diseases to derangement of the *digestive organs*. Although we are not disposed to go the lengths of either Dr. W. or Mr. Abernethy, on this subject; yet, we are convinced, that there is much truth and justice in the doctrines of both these gentlemen.

"Acute rheumatism," says Dr. W., "in its progress, appears to include two stages of disease; the first of which is *gastric fever*, attended by a painful state of the membranes surrounding certain joints, excited by a spasmodic affection fixed on them, in consequence of morbid sympathy, with irritation in the *primæ viæ*. The second stage is formed by an inflamed state of these membranes, created by the continued action of the same sympathetic affinity; which inflammation is occasionally so considerable as to produce effusions into the cellular membrane, and even the bones of the articulation become sometimes inflamed, eventually giving rise to ankylosis, and other diseases of the joints." P. 221.

On these principles, Dr. Wilson employs liberal evacuations from the *primæ viæ*, especially by calomel and antimony; aided occasionally by the neutral salts, particularly the tartrate of potass, or phosphate of soda, "till the signs of a loaded state of the digestive organs shall disappear, or at least, shall have greatly abated. Anodynes, which have the effect of suspending spasm, and of procuring a state of rest, are useful from the beginning; but their usefulness is most eminent after the primary irritation is lessened." P. 223.

Our author employs bark and steel, when the febrile state is subsided. For the local inflammation he trusts principally to topical bleedings and fomentations, "a little more than blood

warm." He also recommends warm anodyne poultices, composed of the common forms, with opium or cicuta added. He is an advocate for calomel and opium in acute rheumatism.

From this disease, the transition to gout is easy. Our author not only supports the idea of a strong affinity between gout and rheumatism, but comes to the conclusion, that both diseases mainly depend on derangement in the digestive organs.

"In contemplating the above premises, it is obvious that there appears no other demonstrable cause of vascular excitement, but that which, by unequivocal signs, is shewn to exist in the digestive organs; therefore, this unavoidable conclusion is likewise obvious, that the febrile paroxysm, excited in the attack of a fit of gout, must be viewed as real gastric fever; and farther, that any other suppositious principle, as inherent in the system, but which is undemonstrable as a primary agent, or remote cause of the disease, may fairly be esteemed as imaginary."

This doctrine, indeed, is very little different from that inculcated in some of the latest and best works on gout. In respect to the treatment, Dr. W. justly observes, that over the predisposition or idiosyncrasy of the patient, medicine has little control; yet, many of the inordinate movements in gout may be soothed and moderated—"especially by evacuations from the stomach and bowels."—Dr. W. considers also, that such medicines as act upon the skin, are extremely beneficial; as the liq. ant. tart. and tinct. opii, accompanied with diluting drinks. The acetate of ammonia also, given in tepid drinks, is useful for the same purpose. In respect to local applications, "the tepid warmth of an emollient poultice, void of every kind of acrimony, is the most suitable; or, tepid emollient fomentations, covering the parts afterwards with warm flannel. To these may be added, topical bleeding with leeches." P. 334.

In the intervals of the paroxysms, Dr. W. recommends the greatest attention to the state of the stomach and bowels, particularly by laxative medicines.

"Of the last, calomel is the most efficient, and ought to form the chief basis of the measures taken for clearing the lower intestines, accompanied with the occasional use of some saline purge."

Emetics, he thinks, are valuable remedies in the remissions of the disease. He condemns the external application of cold, lest "some other organs, which may be next in sympathetic affinity with the seat of irritation, become affected with its influence; and if this shall happen to be any vital organ, the result may be fatal." P. 339.

Dr. W. next proceeds to draw a parallel between gout and erysipelas—a parallel which has been very fully drawn by the continental physicians, as lately translated by the Editor of this Journal. We shall quote the following case:

"A gentleman has had an ulcer on his leg for many years. Along with the ulcerated leg, he is, at the same time, subject to bilious accumulations in the *primæ viæ*; but these accumulations seldom occur without producing a febrile paroxysm, attended by extensive erysipelas around the ulcer; under the influence of which he has occasionally laboured for two weeks at a time, but without any medical aid, further than what he derived from some gentle aperient medicine to preserve his bowels soft. At last, when under one of these feverish fits, he was persuaded to take, though reluctantly, an antimonial emetic, and even to repeat it, followed with a mercurial laxative; by which means he threw off a great quantity of offensive bilious fluid, after which the inflammation and fever very quickly abated. Since this time, as the ulcer on his leg still remains, and continuing still subject to bilious complaints, he has become aware of the approaching illness; and being now convinced of the benefit attending this practice, he has, by seasonable evacuation, saved himself from many a fit of fever and erysipelas." P. 376.

Dr. W. observes, and wisely observes, that irritating matters, and consequently irritation, cannot be expected to be removed from the long line of the digestive organs by a few evacuations, whether of the emetic or cathartic class. Part of the offending matter will often lurk in the duodenum, or some of the sinuosities of the other intestines, while the ordinary discharges by the rectum are suffered to pass along. Additional morbid secretions may also be collected anew—

"Therefore the evacuations ought to be persevered in till the signs of irritation in the stomach shall disappear, and till

the discharges from the rectum shall assume a healthy aspect; otherwise, the portion of vitiated matter which is retained, will assuredly tend to prolong the disease, and disappoint expectations." P. 379.

Angina Pectoris. Dr. Wilson thinks that post mortem researches "have offered no certain information, nor discovered any organic disease within the thorax, which might be referred to as the direct cause of its symptoms." 382.—Here we cannot coincide with our author. The Histories and Dissections of Dr. Parry, and cases which have since been published, afford very satisfactory evidence, that all the symptoms of angina pectoris have *sometimes* resulted from ossification of the coronary arteries. We are ready to admit, however, that all the phenomena of syncope anginosa have *also* occurred, and terminated even in death, without any disease of structure being cognizable to the senses. This must lead all unbiassed minds to the conclusion, that it is sometimes a disease of structure, giving rise to periodical lesion of function—and at others, only a derangement of *function* in the respiratory and circulating organs, leaving no perceptible trace of its previous existence after the thread of life is snapped. In the latter cases, we can only suppose it to be of a spasmodic nature, resulting from arthritic or rheumatic irritation misplaced, and not yet amounting to ostensible organic change in any tissue or vessel. It is in this light that our author views it, and we shall present his ideas on the subject.

"In the course of my own experience, I have seen but a few cases of this kind; but from my observations on these few, I have uniformly discovered, that at the commencement of the disease, the digestive organs were in a loaded state; as also, from farther inquiry, that they had previously been so. But from considering the strong disposition to sympathetic action which subsists, in some individuals, betwixt these organs and the heart, as well as other organs within the thorax, together with a general review of the predominant symptoms, I cannot avoid drawing this inference, that angina pectoris is a disease which also arises from a morbid sympathy, with a primary cause, seated in the primæ viæ." P. 385.

With respect to the particular seat of the spasm, Dr. W.

thinks, that it is doubtless either in the heart itself, or the adjoining great blood-vessels, or perhaps in both at the same time; he also thinks, that from the fixed severe pain under the sternum, a little to the left side, and at times, quite across to the back, the mediastinum may be very materially included in the spasmodic affection, perhaps in a primary manner, as in arthritic or rheumatic translations. It is certain, indeed, that angina pectoris chiefly attacks those of a gouty or rheumatic character. We shall introduce an interesting case in elucidation.

“ April 18th, 1777. Mrs. —, aged 50 years, complains of an intolerably severe stricture extending quite across her breast, attended by a severe pain under the middle of the sternum, which communicates itself to both her arms in a violent manner, but confined to a spot about the middle of the humerus; accompanied with a feeling of suffocation, and a most irregular, soft, intermitting pulse. In this state she continues for the space of ten or fifteen minutes, when the symptoms gradually abate, and she feels free from any complaint, excepting weakness, her spirits being much exhausted. Her appetite for food is impaired; her tongue much furred, with thirst; bad taste in her mouth; weight at the præcordia, and restless nights; her pulse, excepting when under the fit, being natural. She is of a spare habit, and subject to atonic rheumatism in her arms and limbs, and has been repeatedly affected with violent periodical head-aches; she has been complaining in this way for some days. At first, the fits returned twice or thrice in the twenty-four hours, with unequal violence; but she feels them increasing in frequency as well as in violence, and now a recumbent posture is certain of producing them at any time.

“ She had, on some former occasion, been alarmed by the severe operation of an emetic; and I found, at present, that her aversion to it could not, as yet, be conquered. I was on that account left to the effect of laxatives alone, for the purpose of clearing out her stomach and bowels. These were accompanied with opiates, and a blister on her breast. The first cathartic dose produced some bilious stools; and a repetition of it every morning, with a powerful opiate three times daily, had the effect of mitigating the violence of the disease for a

few days; after which it returned with increased violence, both in length and frequency of the fits, accompanied with faintness; her pulse was extremely confused, and she described her feelings of anguish as intolerable. She had now been out of bed for eight nights and days, because any attempt at a reclining posture was certain of producing a paroxysm; on which account she was compelled to spend the whole of her time in her chair, and, notwithstanding the opiates, she slept very little. I now at last prevailed on her to use an emetic of ant. tart. and she discharged a great quantity of acrid, bilious-coloured fluid. On the succeeding night she enjoyed more sleep, but still could not bear a lying posture. On the 27th the fits less severe, and less frequent; she continues the opiates; and on the 28th took a laxative dose. On the 29th relapsed, and took another emetic; threw up much bile, and was again relieved.

“ May 2d, took another emetic yesterday, and has had no fit for twenty-four hours—can sleep in bed. May 4th, has had some slight returns yesterday, and repeated the emetic; discharged more offensive bilious fluid, and is better. May 10th, continues better; her tongue is clean, and other signs of a loaded stomach, &c. gone off. May 29th, has taken stomachic bitters, with the daily use of aperient pills; and by the help of moderate exercise, and restorative diet, remains quite well. She had afterwards repeated slighter attacks of the same complaints, which were always removed by a discharge of offensive matter from the intestinal canal, procured by the same means, and died at last of another disease in the year 1790.” P. 390.

Finally, our author is convinced, from a careful attention to the phenomena of croup, that the primary seat of irritation is in the digestive organs, whence is generated, by morbid sympathy, a spasmodic affection of the glottis, ending in inflammation of the mucous membrane of the larynx and trachea, ultimately destroying the action of the lungs, by rendering them impervious. Acting from these premises, Dr. Wilson asserts, that our great indication is to clear the stomach and alimentary canal, as early as possible, by emetics and mercurial cathartics.

“ Indeed, I believe these medicines to be the most efficient

means hitherto known of speedily and effectually accomplishing, on any occasion, this important object." P. 409.

This plan will often, he thinks, check the disease in its nascent movements, and before actual inflammation has commenced; but we believe that such nascent movements, or in other words, the *spasmodic* stage, is rarely witnessed by medical men; and consequently, it is against the inflammation and exudation from the surface of the mucous membrane that we have principally to guard. Hence, while we take every opportunity of acting on the alimentary canal by emetics and calomel, we must detract blood from the throat, and counter-irritate in the neighbourhood, with all possible expedition and energy.

We have now presented a full analysis of Dr. Wilson's work. Its greatest fault, or more correctly speaking, its misfortune, is the diffuseness of the style. In one fourth part of the space, he might have given the same matter in an infinitely stronger point of view. Indeed, *medical economy* in medical literature is too much neglected. We are fully convinced, that the man who endeavours to diffuse professional knowledge on the easiest terms will, in the end, be the greatest benefactor to medical society. The higher classes have not time to peruse, and the inferior orders have not money to purchase voluminous publications; and hence the stream of science flows in narrow and extremely confined channels. We could illustrate this fact by anecdotes which would be scarcely credited by the well-informed members of the profession, but which are strictly true. We were lately in conversation with one of the first rate medical practitioners in the greatest city of the world, who neither knew the name nor any part of the writings of one of the greatest ornaments of medical science at the present period! It would be equally mortifying and discouraging to the really zealous cultivators of medical science, if they knew the narrow limits in which their labours move. It is true, that medical works take a large *circuit*, in consequence of medical clubs and societies; but the *actual readers* are few indeed!

This, in some degree, is owing to the expansion of little matter into large space, and to the consequent expense of purchase; so that, generally speaking, medical writings are little known, excepting through the medium of Journals.

But to return. The doctrine of morbid sympathy has, in Dr. Wilson, received a strong support and confirmation. Our readers are aware, that the leading points of our author's doctrines and opinions have been delineated lately in another publication, and the coincidence of ideas is a strong proof of the solid foundation on which the doctrines stand. We hope that Dr. Wilson will, in a future edition, compress the matter into less space, and adopt a more precise and philosophical style. There are some inaccuracies of language in the present work; but still it is a very valuable publication, and does infinite honour to its author.

An Essay on the Diseases of the Excreting Parts of the Lachrymal Organs. By WILLIAM MAC KENZIE, Member of the Royal College of Surgeons, of the Medical and Chirurgical Society of London, and Lecturer on the Anatomy and Surgery of the Eye. 8vo. pp. 95. London, Anderson and Chase. 1819.

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THIS essay constitutes an addition to our medical literature of considerable value: the subject of it is particularly interesting, and is by no means well understood by the generality of English surgeons. Under the term *fistula lachrymalis*, various forms of disease have been comprised, differing in their origin, nature, and in the treatment they require; and, as is ordinarily the case, a want of accuracy and precision in the definition of these maladies, has been attended with a want of due discrimination in the measures employed for their relief. The German surgeons excel, on many occasions, in nice discrimination of the various forms of disease which may occur in particular organs, and especially in those affecting the eye, and parts subservient to it. To supply in part the deficiency of information in our language on this point, is the intention of the present work; and it is an object which Mr. Mac Kenzie is well qualified to effect, both from his literary researches,

and his opportunities for personal observation, during a residence for some time in Germany, and especially his experience under the tuition of Professor Beer, of Vienna. Yet Mr. Mac Kenzie is not to be considered in this instance merely as an imitator: he has differed, in some respects, in his arrangement of the subjects of the present essay from Professors Beer and Schmidt, whom he chiefly follows; and advances some original observations of practical utility.

The author distinguishes the diseases of the excreting parts of the lachrymal organs into the following varieties: I. *Wounds of the lachrymal canals.* II. *Erysipelatous inflammation of the parts covering the lachrymal sac.* III. *Acute inflammation of the excreting parts of the lachrymal organs.* IV. *Chronic blenorrhœa of the excreting parts of the lachrymal organs.* V. *Stillicidium lachrymarum.* VI. *Fistula of the lachrymal sac.* VII. *Caries of the os unguis.* VIII. *Relaxation of the lachrymal sac.* IX. *Mucocele of the lachrymal sac.* X. *Obstruction of the lachrymal canals.* XI. *Obstruction of the nasal duct.*

This arrangement is not, we consider, wholly free from objections. Fistula of the sac is always a consequence of some of the other forms of disease above-mentioned, and may accompany nearly the whole of them; it must also vary in its character, according to the causes on which it depends, and requires a relative diversity in the measures for its cure: it would, therefore, probably have been more properly treated of in connexion with the diseases which it may accompany, than in an isolated manner. The comprising of caries of the os unguis amongst the diseases of the excreting parts of the lachrymal organs, is not quite correct. We are also not perfectly satisfied with the making a distinct variety of relaxation of the lachrymal sac; but, on turning to the work of Prof. BEER* on Diseases of the Eye, we find that he also considers it in a distinct manner, under the appellation of hernia of the lachrymal sac, which must be considered as high authority in support of its propriety; although Prof. Beer, as well as Mr. Mac Kenzie, describes it to be a consequence of previous excessive disten-

* *Lehre von den Augenkrankheiten, als Leitfaden zu seinen Oeffentlichen Vorträgen.* Von GEORGE J. BEER. Wien. 1817.

tion of the sac from collection of mucus, &c. in that organ, from either acute or chronic inflammation of its lining membrane. A similar objection may be made to the consideration of *stillicidium lachrymarum* as a variety of disease, regarding it in its connexion with a morbid state of the *excreting* parts of the lachrymal organs. With those exceptions, we consider the arrangements adopted by Mr. Mac Kenzie as systematically proper, and calculated to be of considerable utility in its application in the practice of surgery. A rapid view of the work will render this evident.

We pass over the first section, since it contains no pathological observations of important novelty, nor any therapeutical precepts not indicated by the general principles of surgery.

In the *erysipelatous inflammation of the parts covering the lachrymal sac*, the appearance of the integuments is similar to that produced by erysipelas in other parts of the body. The irritation extending to the *papillæ lachrymales*, causes a contraction of their orifices, and a collection of the tears in the nasal angle of the eye; and, when the inflammation is more severe, affecting the lachrymal sac, nasal duct, and canals, the *papillæ* become so contracted, that the *puncta* are completely closed; "the nostril upon the side affected is dry, and so uncommonly sensible, that the slightest irritation of the *schneiderian* membrane causes violent sneezing." These accidents generally disappear when the inflammation of the integuments subsides; but, the author continues to observe,

"If the lachrymal canals have participated more than common in the disease, the re-absorption of the tears does not take place immediately upon the inflammation subsiding. On the contrary, a *stillicidium lachrymarum* continues, when all the other symptoms have disappeared.

"If the inflammation is severe, and extends beneath the integuments, the lachrymal sac, at the commencement of the second stage, becomes completely filled with mucus, which can always be discharged by pressure. At the same time, the erysipelas ends in a real process of suppuration; the subcutaneous cellular substance becomes disorganized in order to make room for the matter of an abscess: this matter collects between the integuments and the *orbicularis palpebrarum*;

sometimes it makes its way between the fibres of that muscle, penetrates the fibrous layer by which the sac is immediately covered, and comes into contact with the anterior part of the sac, which is already distended by the presence of an inordinate quantity of mucus. At last, the integuments give way in one or more points, and the abscess is discharged. The appearance of the parts is now apt to impose upon a superficial observer. There is the tumor of the sac; there is the fistulous opening of the integuments. He probably pronounces the case to be a fistula lachrymalis, and forthwith opens the sac.

“ Let us distinguish this case from another to which it bears some resemblance, but with which it must by no means be confounded. It may happen that the purulent matter accumulated under the integuments has actually penetrated the anterior side of the sac, which thus comes to be filled with pus received from without; in the production of which its lining, or mucous membrane, has had no share. This latter case, which, for the sake of distinction, may be called spurious fistula of the lachrymal sac, must be carefully distinguished both from the former, in which the sac is entire though distended with mucus, and from those diseases hereafter to be described, in which the purulent matter that fills the sac is the result of inflammation of the lining membrane of the sac itself.”

We have indulged in the foregoing extensive transcription, because it furnishes a good specimen of the author's accuracy of observance, and will show the importance of precise pathological knowledge in the treatment of the disease.

Some observations on the causes, prognosis, and the general and local treatment, follow; from which we adduce an extract, that, with the preceding account, will convey rather a comprehensive view of the most important part of this section of the work.

“ At the commencement of the suppurative stage, the patient must first of all be placed in a warm and dry atmosphere. A dry linen compress ought to be laid upon the affected integuments. Gentle diaphoretics ought to be given. If the symptoms indicate the formation of a subcutaneous abscess, a warm poultice of bread and milk ought to be applied. Still, we must not leave the matter of the abscess to make a way for

itself through the integuments; but, as soon as even indistinct fluctuation is perceived, we must open the abscess with the lancet, in order to save the lachrymal sac, and prevent the formation of a spurious fistula.

“If we are not called till such a fistula has formed, let us beware of all unnecessary introduction of probes and syringes into the sac. By means of a small syringe, the fistula is to be washed out once a-day with tepid water, mixed with a little of the vinous tincture of opium. A small quantity of lint dipped in the same tincture, is then to be introduced into the abscess, but not pushed so deep as to enter the lachrymal sac. After the fistula has healed, the blenorrhœa which may remain is to be treated as shall be explained in the fourth chapter.”

In the form of disease next considered, *acute inflammation of the excreting parts of the lachrymal organs*, the primary seat of the malady is the mucous membrane of the whole of the excretory parts of the lachrymal organs; this is accompanied with

“Obtuse, deep-seated pain, extending to the nose, and even to the eye-ball; a swelling appears in the situation of the lachrymal sac, having the shape of a bean, accurately circumscribed, hard, very sensible to the touch, and affected with stinging pain whenever it is pressed. This swelling becomes gradually red, at last extremely red, and then the least touch is insupportable. The papillæ are shrunk; the puncta are scarcely visible; the absorption and conveyance of the tears into the lachrymal sac, and through the nasal duct into the nose, are completely stopped; and a stillicidium, or discharge of tears and mucus, takes place from the nasal angle of the eye. The nostril on the affected side is at first uncommonly moist, but it soon becomes dry, the inflammation extending to the mucous membrane of the nose. The inflammation affects the caruncula lachrymalis and the conjunctiva, spreading also to the orbicularis palpebrarum, and to the integuments of the lower eyelid. The redness about the nasal angle of the eye, with some degree of swelling even to the cheek, gives to the parts, when viewed at a distance, an appearance as if the integuments were attacked by erysipelas; but, on a nearer examination, the peculiar redness, and all the other characteristics of phlegmonous

inflammation, are recognized; and, in the midst of the diffused discoloration and tumefaction, the circumscribed swelling of the lachrymal sac is evident, not merely to the touch, but even to the view."

The author traces the progress and consequences of this affection in a very lucid and satisfactory manner: the most frequent results, when the inflammation has been severe, are thickening of the mucous membrane of the whole organ, and obstruction of the canals and nasal duct. The abundant secretion of mucus, being collected, distends the sac, induces inflammation and consequent rupture of its paries, and the matter is evacuated externally through the fibres of the orbicularis palpebrarum muscle and the common integuments: the consequence of this is the fistulous opening, described more particularly and distinctly in the fifth section of this essay. Sometimes, even after this accident, the natural passage of the tears is spontaneously re-established, and the opening in the paries of the sac will heal. The latter occurrence may also happen whilst the nasal duct alone remains impervious; when the patient is obliged, several times a-day, to press upon the sac to evacuate its contents. If the wound be permitted to heal whilst the canals and duct are both obstructed, the natural secretion of mucus going on will distend the sac, and induce the disease described in the ninth section under the term *mucocele*, by Mr. Mac Kenzie; or what is, not so appropriately, called dropsy of the sac by Prof. Beer.

A more mild degree of inflammation, or irritation, of the same parts, constitutes the next variety, *chronic blenorrhœa of the excreting parts of the lachrymal organs*. This is the most common of the diseases described in this essay, and is treated by the author in rather an extensive manner. The various causes, constitutional and local, from which it arises, and the consequent diversity in its character and the treatment required for it, render it impossible to convey much information respecting it in an abstract. A bean-shaped tumour, from distention of the sac, and a discharge of puriform mucus through the puncta, for the canals are not obstructed, are the most usual symptoms. Persons of a scrofulous habit are most frequently the subjects of it; and it is usually particularly troublesome, or

solely attacks the patient, during cold and wet weather. The more severe consequences of it are fistula, from attacks of acute inflammation ending in suppuration; induration of the organ; and permanent obliteration of its canals. The obstruction of the nasal duct, in the first instance, merely depends on the tumefaction of its mucous membrane.

This affection is ordinarily much mismanaged by practitioners in general, from a want of correct knowledge of its real nature.

Stillicidium lachrymarum is properly distinguished from *epiphora*; the former depending on obstruction of the excretory parts of the lachrymal organs, the latter on increased secretion of the tears; and, in the former point of view, cannot with propriety be considered as a form of disease.

In the section on *fistula of the lachrymal sac*, the author describes, in a particular manner, the different varieties of ulcers connected with the paries of that organ, and the appropriate mode of treatment under the various circumstances in which they appear.

From the chapter on *caries of the os unguis*, we must transcribe the following passage; it describes what we believe is frequently too true.

“The idea of the frequency of caries of this bone, which, notwithstanding the testimony of Sharp and Janin, has continued to prevail, appears to be founded chiefly upon the mismanaging treatment of surgeons themselves; and, above all, is to be attributed to their rude examination of the parts with probes and other instruments. A patient presents himself with fistula of the lachrymal sac; the idea of caries starts up in the surgeon’s mind, and he forthwith takes a probe in order to examine whether there is caries or not; he penetrates the posterior part of the lachrymal sac, touches the bone with the point of the instrument, which he moves about to this side and to that, in order to make himself sure of what he is seeking for; and at last, distinctly feeling the bone, which he has denuded, he pronounces the os unguis to be carious.”

Caries of the bone but rarely happens, except from fistulous ulcers of the sac, syphilis, and scrofula; and is, consequently, not a subject for any important measures of expressly local treatment.

The *relaxation of the lachrymal sac* is a consequence of its over-distention by collected mucous or puriform matter, in the disease described in the third and fourth sections, which has destroyed its natural contractility; and it becomes distended by its own mucus, often still secreted in preternatural quantity, and the tears collected in it, into a bean-shaped tumor. The integuments covering it are scarcely or not at all discoloured; it is not painful, it yields easily to the pressure of the finger, and never exceeds the size of a common horse-bean. Its contents are discharged by pressure, either by the canals or the nasal duct, according to the direction in which the force is applied.

“ The treatment consists in the use of two distinct means, each of which, as may be seen by the testimony of Pellier and others, is, when used alone, apt to fail.*

“ The first is the compression of the sac; and here let it be observed that the present is the only case in which compression of the sac is useful. In any other disease of that part, this practice would produce the most destructive effects. The compression must be carefully applied, constantly continued, and gradually increased. Machines have been invented for this purpose, but they never fulfil with precision all these conditions. We cannot, by such an instrument as Sharp’s or Petit’s compressorium, the first invention of which we owe to Hieronymus Fabricius, keep up a regular and an increasing pressure: the compressing surface, upon the least occasion, especially during the night, is disarranged; and the patient is hindered from pursuing his business by the presence of such an apparatus. Graduated compresses, then, are to be preferred; over these a firm leather pad, of a proper form, is to be placed; and the whole is to be supported by a narrow roller passing round the head. In this manner, the pressure takes place exactly upon the part which ought to be acted upon; it can be daily increased; the pad cannot, even when the patient is very restless, be shoved aside; nor need such an apparatus prevent

* POTT—*Observations on the Fistula Lachrymalis*. Works. London, 1808. Vol. i. page 252.

PELLIER DE QUENSY, *Cours d’Operations sur la Chirurgie des Yeux*. Paris, 1790. Tome ii. page 207.

him from following his ordinary employment, even out of doors.

“The second part of the treatment consists in the application of some astringent fluid, both to the external surface of the tumor, and to the internal surface of the relaxed sac. A great variety of astringents might be mentioned as proper for this purpose; such as the sulphate of iron or of copper in solution, an infusion of oak bark, or diluted alcohol. The graduated compresses are to be moistened twice or thrice daily with the astringent fluid which shall have been selected. A small quantity also of the same fluid is to be dropped into the *lacus lachrymarum*, and left to be absorbed by the puncta, the patient being laid in a horizontal position, and the compresses sometimes relaxed, but not removed.

“Such is the method of curing a disease, which I have shuddered to see treated by incision of the sac, and the passage of various instruments through the nasal duct; operations which are to be regarded as the more severe in such cases, because they are wholly unnecessary. As for the Anelian injections, the direct injury of the papillæ, and the over-distention and consequent atony of the puncta, so that they can no longer absorb the tears, are only some of the bad consequences of such a practice. Such injections in this particular case would be attended also by a momentary over-filling of the sac, which would be just undoing with the one hand what we were effecting with the other.”

In the *mucocoele of the lachrymal sac*, the tumor is accompanied with a purplish colour of the integuments, and it often attains the size of a pigeon's egg; it is very firm to the touch, and its contents cannot be evacuated by pressure, because the canals and ducts are both totally obstructed.

“The colour of the integuments in *mucocoele* has led some authors to describe this disease under the name of *varix of the lachrymal sac*; and the hardness and size of the tumor, added to its colour, have frequently led those charlatans, who were formerly but too often intrusted with the care of the diseases of the eye, to extirpate the lachrymal sac affected with *mucocoele*, under the idea that they were removing a cancerous tumor.”

In the appropriate treatment of this important form of disease, the question is not whether the tumor only can be removed, for this can always be effected by laying open the sac, and turning out its contents: the important question is, whether or not the natural passages can be restored.

Obstruction of the lachrymal canals and nasal duct, are the affections best understood by the English and French surgeons, being those to which their attention has been chiefly directed in their views of the disease of the organ under consideration; yet the observations of Mr. Mac Kenzie on these subjects will be perused with advantage.

Our observations on this essay have occupied a much greater space than we usually devote to so small a work; but this has necessarily arisen from the very concise manner in which the author has himself treated the different subjects it embraces: he has effected this as closely as is consistent with perspicuity of pathological description and practical utility. This is a circumstance which will contribute much to favour the benefit that will ensue from his labours, by rendering general the possession of this work by practitioners of surgery. This is no unimportant merit, at a time when productions in medical literature are so numerous, and in general so expensive, as they are at the present period.

We hope to see Mr. Mac Kenzie pursue the course he has here commenced: there are many other points in the pathology of the eye, and the parts subservient to it, on which he might adduce some illustrations, both novel and important to English medical literature.

The Elements of Chemical Science. By JOHN GORHAM, M. D. Member of the American Academy, and Professor of Chemistry in Harvard University, Cambridge. *Homo naturæ minister et interpres.*—Bacon. Vol. I. pp. 556. Boston, Cummings & Hilliard, 1819.

[From the North American Review, for June, 1819]

THE rapid improvements which have been made in the science of chemistry during the present century, have rendered

it almost impossible for any to keep pace with it in its discoveries and revolutions, except those who have made it a particular object of attention. Never before, probably, have the exertions of so many individuals of the highest capacities and most persevering industry been at once concentrated upon a single subject; or the zeal and ardour of the philosopher been so entirely seconded by liberality and patronage. Chemistry has been the most popular science of the age. The novelty and splendour of some of its discoveries, the almost magical effect produced by some of its operations, and the readiness with which it explains so many of the ordinary phenomena of nature, have served to attract and charm minds but little disposed to relish its deeper investigations. This was particularly the case, whilst the theory of Lavoisier continued to be received in the chemical world. Its beauty and simplicity, the facility with which it was explained and understood, and its almost perfect applicability in accounting for the operations of nature, recommended it to the attention of general observers, as well as of the philosophical inquirer. But the revolutions which in the course of a few years have been effected with regard to some of its most important principles, by rendering chemistry more complicated and abstruse, have lessened very much the fascinations which it formerly possessed. It is now no easy matter—such has been the progress of discovery—to comprehend perfectly the refined speculations of the science, or the new arrangements and relations which have been pointed out in the different forms of matter, by recent investigations. A tolerable knowledge of chemistry cannot now, as it could once, be obtained by attending a few lectures, performing a few experiments, and studying some short and familiar exposition of its principles. It has now assumed somewhat of a more repulsive appearance. Such indeed are the changes which have taken place, that those elementary works which were in common use, have become obsolete; or been mutilated and disjointed by the unskilful incorporation of new discoveries.

Dr. Gorham has performed a valuable service to the University with which he is connected, and to the public at large, in the task he has undertaken; and which he has so far ac-

accomplished with great accuracy, industry and ability. We have been much in want of a book of the kind he has presented to us, which should afford a general, but at the same time, accurate and scientific view of the subject, without entering into those minuter details of practice, which are dry and revolting to all but the professional student. Upon the general principles of the science, upon those points particularly, which are applicable to the phenomena of the external world, with which we are most conversant, we are presented with full and ample discussions; whilst upon those which relate chiefly to the operations of the laboratory, and the manipulations of the practical chemist, the author has taken a narrower range, and confined himself to the most essential particulars.

In systems of chemistry the synthetic method has been most frequently adopted; though it has been often doubted, whether the analytic may not be better adapted to convey a knowledge of its principles.

‘In a work of this kind,’—says Dr. Gorham,—‘a refined and complicated system is not necessary. The plan which has been adopted is simple, and such as appeared to the author the best calculated to present to the reader in a natural order the series of connected facts. In elementary works, he believes that it is most expedient to commence with the general principles of the science, and in illustrating the doctrines of chemistry, it is not necessary that the reader should be intimately acquainted with the bodies by which these doctrines are to be demonstrated. The subjects, therefore, in the following pages, have been divided into two parts. The first part is devoted to the general laws of the science, and to the properties and modes of action of the powers or agents which are concerned in the production of chemical phenomena. In the second part are detailed the properties and relations of ponderable bodies and their compounds.’ *Pref.* p. x.

The method adopted by Dr. Gorham we believe to be more unexceptionable, than that of works upon chemistry in general. Yet we conceive, that for the majority of readers, the best method of instruction would be, to follow, as nearly as possible, the course which the mind takes in investigating. The syn-

thetical arrangement, strictly carried into effect, must, it appears to us, be completely unintelligible except to an adept in the science. It is certainly desirable, in the acquisition of all learning, that we should go through the same processes of mind in studying the observations or discoveries of another, as if we had made them ourselves. We should, as far as possible, teach, or seem to teach ourselves. That which we have discovered by our own exertions is most firmly impressed upon our minds; and the same, though in a less degree, is true, if we follow the steps of the discoveries of another. By pursuing a train of investigation, we make it our own; and the impression of a fact thus acquired is far more lively and permanent, than if we retained it singly in our memory; because in the former case, it is connected with a train of associated ideas, by which it is more easily recalled into the mind than if it stood insulated and alone. Who does not receive a clearer notion of the relations of oxygen to the metallic oxides, from an acquaintance with the precise and logical experiments of Lavoisier, than he would from a dry detail of the simple matter of fact? And although it would be impossible to enter into minute analyses and histories of experiments in an elementary work, yet the general method is possessed of similar advantages; and it is for this we contend.

There are two classes of persons who pursue the study of chemistry—and indeed the same is true of every other science—those who wish merely to acquire such a knowledge of its principles and operations, as will enable them to understand the constitution and relations of the objects about them, and the causes of the different phenomena in the external world;—these study only the chemistry of nature;—and those who follow it professionally, who have more regard to its practical details and manipulations, than to its connexions with the operations of nature. These, if we may so speak, study principally the chemistry of the laboratory. Now there ought to be a great difference in the modes of instruction, where the objects to be attained are so different; and there is perhaps no question, that the synthetic method is best adapted for the education of the professional chemist. He should study the

science before he studies nature; and become master of its artificial and technical forms, before he applies them to the elucidation of any thing out of his laboratory. He should become familiar with the effects and changes wrought by his retort and crucible, before he would penetrate into the mysterious secrets of the air, the earth, and the ocean. But the opposite method seems, as we have intimated, better fitted for the purposes of the general scholar, who seeks only knowledge enough to enable him to comprehend the chemical economy of our globe, and the nature of those forms of matter with which we are most conversant.

Every advance in discovery carries our attention further and further from those substances with which we are most familiar. The elements of the ancients have long since been decomposed; and many of the substances into which they were separated, have been again and again subjected to the same process. If then we commence at the point where discovery has terminated, it is obvious that we immediately enter upon the consideration of objects perfectly new, and which, in fact, have no real, separate existence in nature, but are only found as the elements of other matter. As the science advances, then, we have to commence at greater distances from the ultimate purpose of our research, and to go through with an investigation of substances which have, in a certain sense, only an artificial existence, before we arrive at the consideration of those which it is our particular object to understand.

We think, then, that a general knowledge of the chemical constitution of bodies, and of the relations of the elementary substances to those compound forms of matter with which we are familiar among external objects, is best communicated by that method which it has been our object to recommend. The method proposed has also this excellence, which is of considerable importance, that it gives an immediate interest to the very commencement of the study; it awakens the curiosity and rouses the attention at once. We might be easily induced to listen to an account of the nature and composition of water, or of atmospheric air—substances with whose properties we are perfectly acquainted—when we should be completely indifferent about the history or description of oxygen, hydrogen,

or azote—names, of which we had never heard. The student, at the outset, has no knowledge even of the existence of these bodies; and their properties are unlike those with which he has been usually conversant. He is obliged to obtain the subjects of his investigation by long and tedious processes; for they are seldom found in nature in a separate state; and it is only after a careful examination of their various combinations, that he understands the constitution of those substances with respect to which his curiosity is principally excited.

The fluctuating and progressive state of chemical knowledge is likewise an objection to the synthetic method of instruction. Till the science has been perfected, no arrangement can be adopted which will not be liable to constant variations, so long as new discoveries are made, and new views are taken of the ultimate composition of different species of matter. A complete and permanent arrangement upon this plan can be formed only when we know every thing within the limits to which human inquiry may extend, respecting the subjects about which it is employed. But this inconvenience, arising from the imperfection of our knowledge, is much less as it respects the opposite system. The forms of matter are always the same, although our views of their constitution may be constantly changing; and if they are taken as our guides, no changes in chemical theory can ever introduce confusion or disorder into a system founded upon this basis.

The first part of the work under consideration consists of five chapters, which treat of the general laws of the science, and of the powers concerned in the production of chemical phenomena. These powers are, attraction, including the attraction of cohesion, and chemical affinity, caloric, light, and electricity, to each of which a chapter is devoted; and the fifth is occupied in a discussion of their general nature and mutual relations. We cannot pretend to give any abstract of the very valuable matter, relating to these difficult but interesting subjects. Modern researches have given us new views on most of the important points of theory with regard to them; and Dr. Gorham has collected a full and able account of the present state of our knowledge, and of the discoveries and improve-

ments, which have made this one of the most curious and interesting departments of human science.

These changes have been principally brought about by the agency of electricity in chemical investigation; and the introduction of the Galvanic battery into the laboratory has formed an epoch in the history of the science, and given a new direction to the labours and studies of the chemist. The effects of this instrument are manifested in a variety of ways; in the production of an intense degree of heat in various bodies, violent shocks upon the human system, and a chemical decomposition of the most refractory substances; and these effects form some of the most wonderful exhibitions of the powers of matter which are afforded by human ingenuity.

When the voltaic apparatus, as it is usually denominated by chemists, is properly arranged,

‘It will be found that the two extremities or poles are in opposite electrical states, the zinc always being positive and the copper always negative.’—‘When the hands are well moistened and are brought into contact, the one with the positive, the other with the negative pole, a shock will be experienced proportional to the number of plates; the shock from a battery of four hundred plates is exceedingly violent. If the wires be terminated by cones of dry charcoal, made from box-wood; on forming the communication, sparks will be produced, and part of the charcoal will become red hot. When pieces of this substance, about an inch in length, and $\frac{1}{6}$ of an inch in diameter, were connected respectively with the positive and negative poles of the great battery of the Royal Institution, and brought very near each other, a bright spark was produced, and more than half the volume of the charcoal became ignited to whiteness; and, by withdrawing the points from each other, a constant discharge took place through the heated air, in a space equal at least to four inches, producing a most brilliant ascending arch of light, broad and conical in the middle.’

‘The temperature excited in the circuit of batteries which are composed of a very large number of small plates, or of a small number of large plates is intensely high. The power to ignite bodies was almost equally apparent in a battery consist-

ing of two thousand double plates, each exposing a surface of thirty-two square inches; and in Mr. Children's arrangement, each triad exhibiting a surface of thirty-two square feet. By the former, platina, one of the most infusible of bodies, was melted in the arch of flame as readily as wax in the flame of a common candle; quartz, the sapphire, magnesia and lime, all entered into fusion; fragments of diamond, and points of plumbago and charcoal rapidly disappeared and seemed to evaporate. By the latter, $8\frac{1}{2}$ feet of platina wire 0.44 of an inch thick were heated red hot; in small quantities it became bright red and melted at the ends; some of the most refractory oxides were reduced, and the most difficultly fusible metals melted into globules. The metals, when reduced to very thin leaves, do not require for their ignition or combustion that the power should be very high. Gold and silver leaf may be consumed, the one giving forth a brilliant yellow, the other a beautiful greenish white light, by connecting them with the poles of a battery composed of 100 double plates of three or four inches.

‘The chemical effects of the voltaic apparatus are wonderful. Its operation is gradual, but continued, and no form of matter if compounded is able to resist its action. Compounds in the liquid state are decomposed with facility; but even insoluble compounds and the hardest and most solid aggregates are finally resolved into their elements by the decomposing power of voltaic electricity.’ pp. 178—180.

By the introduction of wires of gold or platinum, connected with the two poles of the battery, into a vessel of water, this fluid is immediately decomposed, and its elements extricated; the oxygen being disengaged from the zinc or positive pole, and the hydrogen from the copper or negative; and this will take place when the distance of the wires through the water amounts to three feet. If, instead of these metals, those be employed which have a strong attraction for oxygen, the hydrogen only will be liberated, the oxygen combining with the metal; and on the other hand, if some other of the metals be employed, tellurium for example, no hydrogen is evolved at the negative extremity of the battery, but oxygen only, at the positive, whilst the appearance of the metal is

changed, and it is believed that a compound is formed of hydrogen with the metal, which may be called a hydruet. By a similar arrangement, many other substances may be decomposed, as ammonia, the mineral acids, the metallic and neutral salts; and in all these cases the elements evolved correspond to the known composition of the substance subjected to experiment.

In the decomposition of these substances, the acid is always separated at the positive, and the alkali or metal at the negative end of the battery; and this, even under the most unfavourable circumstances for the production of such effects.

‘ Thus, if three cups of agate or of gold be connected with the apparatus, the middle being filled with a solution of sulphate of potash, and the two others with pure water, and they be connected with each other by moistened amianthus, it will be found after a time, that the cup, positively electrified, will contain sulphuric acid, and the cup negatively electrified, potash, although no traces of either of these substances could be perceived in the water previous to the experiment. If the two extreme cups be filled with an infusion of red cabbage instead of water, the portion connected with the zinc end of the battery will become red, and that united with the copper end will be changed to a green; effects which are produced respectively by acids and alkalies.

‘ So powerful is this apparent attraction of acids to the positive end, and of alkalies or bases to the negative pole, that in passing to the respective extremities of the battery, it was proved by Sir H. Davy, that they might be made to proceed through, without combining with, substances, which in ordinary circumstances exert strong affinities. Thus, when solution of sulphate of potash was put into the cup connected with the negative end, liquid ammonia into the middle cup, and pure water into the cup in which was immersed the wire from the positive surface; on completing the circuit, and allowing the apparatus to remain in that state for some time, sulphuric acid was found in the water; when the order was inverted, the sulphate of potash being in the positive cup, acid in the middle, and water in the negative cup, the base of the salt, or potash, was discovered in the water. In the first instance, therefore,

acid matter was transmitted through ammonia, and in the second, alkaline matter or potash passed through an acid, without chemical union, notwithstanding the particles had freedom of motion, and are known to possess strong mutual affinities. By the same arrangements, acids and alkalies may be made to pass through coloured vegetable infusions without producing their characteristic effects.' pp. 185, 186.

The results obtained by means of the voltaic apparatus, and the great influence which this form of electricity was thus found to possess, in modifying and even neutralizing chemical affinities, suggested to Sir Humphrey Davy the idea of attempting, by the same means, the decomposition of the fixed alkalies; and the consequence was one of the greatest discoveries of modern science. These substances had been supposed to be compounds; but there had been no well grounded opinion formed with respect to the nature of their ultimate composition. By subjecting them, however, to the influence of a powerful battery, oxygen was given off at the positive pole, and at the negative were observed small globules of a metallic lustre, resembling mercury, which combined rapidly with oxygen, if exposed to it in any of its forms, and reproduced the pure alkali. By employing substances, into whose composition oxygen did not enter, to receive and contain the results of his experiments, this great chemist was enabled to obtain for observation quantities of the new metal; which was thus proved to be the base of the alkalies; and potash and soda to be consequently of the class of oxides. This discovery was easily extended; and, partly by actual experiment, and, partly by analogical reasoning, it has been concluded, that the alkaline and other earths are similarly constituted; that they are all oxides, resembling in composition those bodies acknowledged to be such, and having the same chemical relations and powers. Their bases have accordingly been ranked in the class of metals, with names, such as potassium, barium, &c. derived from those of their oxides, and corresponding in etymological structure to those of the other metals.

Much of the history of voltaic electricity, as applied to the operations of chemistry, and indeed many other of the inves-

tigations of modern chemists, have thrown considerable doubt on the prevalent opinions, respecting the nature and agencies of the powers by which the operations of nature are carried on. The causes of what we call the effects of caloric, of light and electricity, are generally believed to be fluids, unconfined and imponderable, tending to diffuse themselves through space, capable of being transmitted from one substance to another, and of being accumulated in some bodies, and diminished in others, according to their several capacities, or the influence of a variety of circumstances. The theoretical speculations of philosophers have commonly proceeded upon this assumption; and it has not been long, that different opinions have attained any considerable currency. Such a belief is, however, at variance with many well known facts, and has been relinquished by some of the most distinguished chemists. It is not a great while since the mechanical phenomena of the universe were explained upon a similar hypothesis; and gravity like heat was supposed to be an all pervading fluid. Even within a few years, a similar doctrine has been hinted at with regard to magnetism; and it is obvious, that explanations might be given of chemical attraction or affinity upon the same principle. But such hypotheses are now relinquished as chimerical. Gravity and affinity are believed to be the result of certain properties or powers of matter, by which it is enabled to maintain certain relations, and the phenomena of magnetism, notwithstanding the exception to which we have just alluded, are now generally accounted for upon a similar principle. But the old method of explanation, has, for no satisfactory reason that we can perceive, been retained with respect to heat, light and electricity;—and their operations have been supposed to be performed by the interference of a specific sort of matter.

Yet where is the necessity of this interference of subtile and imponderable fluids? Does it in fact explain the phenomena in question any better than they can be explained without it? And if not, is it not unphilosophical, gratuitously to suppose their existence? There has always been, in the early stages of science, a great deal of this sort of philosophizing, which removes the difficulty it cannot solve, one step further off. How are the questions to be answered, when we

come to inquire, by what agents the revolutions of those subtle and imponderable fluids are effected, and by what causes they are made to perform the various offices assigned to them? The fact is, nothing is gained by these assumptions; one after another may be made without end. There have been some speculations with regard to electricity, which have introduced no less than two or three different modifications of each of the two opposite fluids, upon which its phenomena were imagined to depend. As science becomes more refined and genuine, such doctrines are rejected; and we have no doubt, when the laws of chemistry are as well understood as those of other branches of physical science, that its speculations will be as little encumbered by the relicks of former hypotheses.

It may, perhaps, be thought, that we consider the subject too curiously; but we really conceive that analogies sufficiently close exist between these different powers, to make it worth inquiry whether they are not all to be explained upon similar principles. To take caloric for example, do not its laws and those of gravitation resemble one another in many striking particulars? All the mechanical motions of the universe, we take it for granted, depend upon the power of gravitation, acting under different modifications and in different directions. The momentum acquired by a falling body is an accumulation of this power—as it may acquire an accumulation of caloric by percussion, friction, &c.—and it will produce motion in another substance, or raise the opposite scale of the balance, in the former case; as in the latter it will raise the mercury in the thermometer. The accumulated power of motion, like caloric, is also communicable, and tends to an equilibrium. If a body fall against the earth or some other fixed substance which is unelastic, its motion is destroyed; as a heated body is quenched at once in a large quantity of water. But if, on the other hand it impinge against a small moveable body, its power is partly transferred, and both bodies move on with a force exactly proportional to their size and quantity of matter. How nearly does this resemble the laws of the distribution of heat. If, however, the substance against which another falls be elastic, the moving body rebounds. The direction of the power on which its motion depends *appears to be* changed

or reflected, for the body itself is reflected from the elastic, precisely as light or heat is from a polished surface. We may find also without difficulty an arrangement by which we may produce an effect upon the gravitating power of matter, similar to that by which the light and heat of a large surface are concentrated in a single point. If we suppose a cone made to rest upon its apex, its whole weight is collected at that point. Now, in this case, the gravitating power of every particle out of the axis of the cone, is made to operate in a different direction from that in which it naturally would—i. e. towards the centre of the earth—and to assume a tendency towards the apex of the cone, as parallel rays of light or heat are collected in one point by the intervention of a lens, or by the reflection of a mirror.

The analogy between the laws of these powers of matter appears to us at least sufficient to render it probable that their nature is similar, and that if the phenomena of the one can be consistently explained without the intervention of any separate and specific material principle, those of the other may be also. But although we readily admit the probable immateriality of caloric, yet we cannot subscribe to the detailed explanation of this doctrine which has been advanced by Sir H. Davy.

‘It is assumed,’ says Dr. Gorham in his account of this theory, ‘that in solids the phenomenon of heat results from a vibratory or undulatory motion of their particles, the temperature varying with the spaces between them, and the intensity of the vibrations; that in liquids and gravitating elastic fluids, the vibration is accompanied with a motion of the particles around their own axis, those of gaseous matter being performed with the greatest velocity; and that in etherial fluids or radiant matter, the particles move around their own axes, and separate from each other, penetrating in right lines into space. Temperature may be conceived to depend upon the velocity of the vibrations; increase of capacity upon the motions being performed in greater space; and the diminution of temperature during the conversion of solids into liquids or gases, upon the idea of the loss of vibratory motion, in consequence of the revolutions of the particles upon their own axes,

at the moment when the body becomes liquid, or aëriform, or from the lessened rapidity of vibration, from the motions being performed in a greater space.' p. 216.

Now all this is purely hypothetical; it could not from the nature of the subject be otherwise; and it is besides somewhat obscure. We are not called upon to enter so deeply into the hidden processes of nature. If we can render it probable, that the supposition of the existence of a peculiar species of matter is without foundation, we are not therefore obliged to show how things can be managed without it. The burden of proof lies upon the opposite side. The production of high temperatures by percussion and friction, and also by chemical combinations and decompositions, in which there is no condensation, affords a principal argument against the material nature of heat. Yet this has no bearing upon the opinions of Sir Humphrey Davy; but is simply of a negative character, tending merely to show that the received doctrines do not explain all the phenomena in question.

In considering the nature of caloric, we ought to throw out of view the relation it bears to our own sensations, which has in fact nothing to do with the question. These are immediately excited by a change in the state or actions of the extreme vessels, and not by any matter communicated from the heated body. The same sensations may be produced by substances of a temperature lower than that of the body, which have the power of exciting a similar action or change in these vessels. We refer to the operation of caustics.

Putting aside then the power of heated bodies upon our feelings, what other powers do they possess, by which they are distinguished from substances at the ordinary temperature of the atmosphere? The effect of a high degree of heat seems to be an increase of chemical energy, and an altered state of chemical affinity. If a piece of metal be heated to a certain degree, its tendency to combine with oxygen is augmented, and if it be exposed to the influence of the air it becomes an oxide. Here is a certain change produced in the chemical affinity of this substance; but at another degree of the thermometer, the same oxide which was thus formed becomes decomposed, and the metal is restored. Here is another

change in its chemical affinity. We perceive changes in chemical affinity, and we perceive nothing more. Expose the same metal, at any temperature, however low, with which we are acquainted, to the action of nitric acid, and similar changes will take place. But, in the last mentioned experiment, heat will be generated. The power, then, which produces the sensation of heat, may be considered merely as the result of the chemical state of certain bodies, of the action of their affinities, and not as a separate substance, the cause of that state and of the action of these affinities. Thus an electrical discharge, which is in itself possessed of no temperature, is capable of setting fire to an inflammable body;—that is to say, electricity, a powerful chemical agent, becomes the cause of a series of processes carried on in the combustible substance, in consequence of which, heat is generated. It is obvious, that upon the principles just stated, the sensible effects which take place in the common cases of combustion, are to be explained, not by the supposition of the extrication of caloric, a separate substance, but as produced, without any such intervention, by the chemical operations which are going forward.

The phenomena of light are certainly better explained, upon the theory of radiant particles issuing from the luminous body, than those of caloric are, upon the material hypothesis. But we have no evidence of the existence and nature of light except by its immediate effects upon our senses. Light is only a power in external objects of affecting our organs, so as to produce certain sensations; and the same sensations may be produced in circumstances where there can be no possibility of the presence of radiant light; as in passing the shock of a Galvanic battery through the head, a flash is distinctly perceived. But whatever doubts there may be with regard to the existence of light, as a separate substance, yet it must be confessed, that the subject is too little understood, to enable us to form any consistent and satisfactory hypothesis.

There seems, till of late, to have been little or no doubt among philosophers, that the phenomena of electricity were caused by the operation of a peculiar fluid or fluids, which pervaded all matter; and the respective theories of Dufay and Dr. Franklin, founded upon their several assumptions, have,

one or the other, been received by all men of science. The principles of these theories are sufficiently familiar to most of us.

‘ But an hypothesis differing essentially from them has been stated by Sir Humphrey Davy. Electricity is conceived by this distinguished Chemist to result from the general powers or agencies of matter, and its phenomena to be displayed in the attractions and repulsions between different bodies or their particles. In these cases they assume a polar arrangement, and the opposite extremities are dissimilarly electrified, the positive pole of one being opposed to the negative pole of the other. The effect is proportional to the violence with which the electrically attractive and repellent powers are exerted, and may be connected with the motions of the particles of the substances affected. The difference in what has been called the conducting powers of bodies, seems to depend entirely upon the different manner in which they receive the electrical polarities, or in which their parts become capable of communicating attractive or repellent powers to other matter. When an excited body is brought into the neighbourhood of an insulated conductor, the air being interposed, the nearest point of the conductor becomes dissimilarly electrified, the remote extremity similarly electrified, and the middle is in a neutral state. The conductor here acquires polarity with ease, and exhibits but two poles, and this polarity is *induced* by the proximity of the electrified body. The polarities of the two bodies may continue to be exalted, until they are annihilated through the air, producing what is commonly called an electrical discharge. The phenomena of sparks, discharges, and accumulated electricity, depend upon this law. The particles of different bodies are also inferred to possess inherent, electrical energies, and it is considered as not improbable, that the same arrangements of matter, or the same attractive powers which place bodies in the relations of positive and negative, i. e. which render them attractive of each other electrically, and capable of communicating attractive powers to other matter, may likewise render their particles attractive, and enable them to combine when they have full freedom of motion. Hence electrical and chemical phenomena may de-

pend upon the same power; in the former case acting upon masses, and in the latter upon the particles of matter.' p. 224, 225.

These principles, in general, appear to us to afford the best explanation of the phenomena of electricity; though the theory just stated is tinged in a considerable degree by that fondness for minute and intricate specifications respecting the operations of nature, which is characteristic of the speculations of its celebrated author. There are many facts which tend to confirm the latter part of the passage just quoted, and to show the near connexion which exists between electricity and chemical affinity.

‘Most of the substances, which act distinctly upon each other electrically, are such as act chemically when their particles have freedom of motion; this is the case with sulphur and the metals, with acid and alkaline substances; and the relations of bodies are uniform; those which have the highest attractive powers being in the relation of positive in arrangements in which chemical changes can go on.’ p. 203.

‘The connexion existing between electrical phenomena and chemical changes, is likewise evident in the general phenomena of the voltaic battery. The most powerful combinations are formed by substances that act chemically with most energy upon each other; and such substances as undergo no chemical change in the combination, exhibit no electrical powers.’ p. 204.

These circumstances, and a variety of others which might be brought forward, lead almost inevitably to the conclusion, that electricity and chemical affinity are identical; that the phenomena attributed to them are the results of the same principle acting for different purposes and in different relations; and when viewed in connexion with other facts, afford likewise much ground for the belief, that heat, light and magnetism are also different exhibitions of the same cause, acting under various modifications, and producing different results, according to the nature of the substance which is operated on.

‘An intimate connexion appears to subsist between electricity, light and caloric. As bodies require to be raised only to a

certain temperature to become luminous, so it is only necessary that the electrical excitation should be increased to a certain point to render them radiant. The effect of lightning in setting fire to combustibles is well known. The electricity excited by friction will inflame combustible liquids, gun-powder and the metallic exploding compounds. Transmitted in large quantities through the perfect conductors, it melts and dissipates them in globules. The temperature produced in the circuit of the batteries of Mr. Children and of the Royal Institution of London, was probably equal or nearly so to that of the flame of the oxy-hydrogen blowpipe, which is the highest that can be effected by art. Electricity even of low intensity is accompanied with light. The spark is familiar, and the star which is sometimes observed stationary upon the point of a conductor, is brilliant and well defined. A variety of bodies, when electrically excited, become phosphorent, and non-conductors subjected to friction are rendered luminous. The colour of the light is not always the same; it varies according to the intensity of the excitation, the distance to which it is to be transmitted, and the nature of the medium which opposes the action. In low degrees of excitation, the colour is red; in proportion as they are exalted, it becomes purplish, and when intense, white with a shade of violet.' p. 232, 233.

Wherever the electrical agency is supposed to operate with the greatest force, there light and heat are proportionally developed, as in the experiments related p. 179 of the work under review, of which we have given an account. Galvanism, as we have previously remarked, is capable of producing the sensation of a flash of light when the eyes are entirely closed; and the dry points of the wires connected with the extremities of the battery produce a feeling of heat in the skin, although their temperature is not raised. These powers of generating, as it would seem, light and caloric, by means of the electrical apparatus, lead to additional doubts of their material nature—for if material, whence is the immense and long continued supply derived in some of the operations of the galvanic battery? Or at least they imply, in all their phenomena, the operation of only common ultimate principles. For if it be argued in defence of the old hypothesis that the heat, &c. thus developed,

is caloric, set free by the chemical operations going on in the troughs of the battery, and which are probably necessary to its electrical powers—still, how is this caloric conveyed along the wires of the battery without raising their temperature, and brought into operation only, when their extremities are made to communicate?

As electricity produces heat, so too the variations of heat produce electricity. It is developed by changes of temperature and of form; by the cooling, for instance, of melted sulphur, by the production and condensation of vapour, and by heating certain mineral substances, which in their ordinary state exhibit no signs of electricity.

The light, which is a consequence of electrical operations, obeys the same laws as that of the sun, and is separable into the same elements by the prism. There is also a farther resemblance between the chemical powers of the different kinds of rays, into which the solar spectrum has been divided, and those of electricity. ‘The rays at the red end, in their chemical powers, tend to burn bodies or combine them with oxygen; those at the opposite end tend to restore inflammability to bodies; and negative electricity, which exercises the same function, produces hydrogen gas from water, and the inflammable bases from their combinations.’ In addition to all this, the influence which both light and electricity have in the production of magnetic power, points out another relation between them, and of both to that inscrutable principle on which the phenomena of magnetism depend. There is no doubt that they are both capable of communicating to iron, under certain circumstances, the polarity of the magnet.

Now what is the conclusion which most naturally follows from all these facts? Is it not this—that the phenomena of affinity, caloric, light, electricity and magnetism are all equally the consequences of certain laws of matter, by which the intimate constitution of substances is maintained, and their chemical revolutions effected; which operate in these different ways, and produce such various results, according to the circumstances of the operation, the nature of the bodies operated upon, and the nature of the substances which are the recipients of the effects? Thus if the stream of galvanic power be made

to pass through a wire of platina, intense heat is produced; if through a quantity of water, a solution of a neutral salt, or some other compound substance, the result is a chemical decomposition; if the subject be a thin metallic leaf, an opposite effect is produced, and the metal combines with oxygen. In some substances, friction is capable of producing an intense degree of heat; in others an excitation of electricity. These are a few exemplifications of this principle, but many others have occurred in the course of our remarks.

The second part of this work relates to the properties and relations of ponderable bodies, &c. They are divided by Dr. Gorham into the organized and unorganized, the former including those substances which are, or have been, parts of some living system; the latter, the inert mass of common matter. The same elements, however, enter into the composition of both, although they are combined by different laws, and for different purposes. The elementary substances described amount to fifty-two, but the existence of a few of these is somewhat hypothetical; and the number is continually varying with the progress of discovery; since it is one of the first principles of the science, that every substance is to be regarded as simple till proved to be a compound.

The elementary substances are arranged by Dr. Gorham under three classes; 1. Supporters of Combustion containing four, 2. Inflammable or acidifiable unmetallic bases containing seven, and 3. Metals containing forty-one individual species. The two first classes only are described in the present volume. Under the sections relating to the several elements, are included accounts of the principal combinations which they form with each other.

Lavoisier founded his system of chemistry, principally, upon the relations which oxygen was supposed to bear to the other elementary bodies; the influence it exerted in combustion; and upon the properties of those substances of which it formed a component part. It was believed to be the only supporter of combustion, and the principle on which the acids, whose composition was then known, depended for their peculiar properties. At the time this theory was promulgated, it was almost without exception applicable to the phenomena to which it re-

lated; and modern discovery has not so much tended to prove that its doctrines are false, as to narrow the limits within which they are to be applied. It has in the first place brought to light other supporters of combustion besides oxygen; or to speak more accurately, other elementary substances analogous to oxygen in most of their properties. In fact, neither of the substances included in this class is absolutely necessary to combustion; for, according to modern views, its phenomena are merely the consequence of intense energy of combination. If substances with strong chemical affinities combine, there is often an evolution of heat and light, whether any of the supporters be present or not. Some of the inflammable bodies themselves in their turn become fully as much supporters of combustion, as chlorine or iodine. There is a gas lately discovered called cyanogen, the basis of the celebrated prussic acid, composed of 46.78 carbon and 53.22 azote, which is capable both of being inflamed itself, and of supporting combustion in other substances. When kindled, it burns with a bluish flame, producing azote and carbonic acid; but if potassium be introduced into it, and heated, the metal takes fire and burns also with flame. The case is similar with respect to sulphuretted hydrogen. Though inflammable itself, it supports combustion in potassium, which will take fire in it and burn with great brilliancy. Unless the definition of combustion be confined within the limits of Lavoisier, there are no particular elements that we can distinctly class together as supporters of that process. Still there are sufficient analogies between the substances thus classed together in the present work, to authorise their arrangement in the same division; although there may be some doubt about giving them the distinctive title of supporters of combustion.

The views of the old theory respecting the constitution of the acids, were still more partial. It is generally believed by modern philosophers, that hydrogen, as well as oxygen, is capable of acting as an acidifying principle, when combined with a base; and that all those substances called supporters of combustion, form acids in union with hydrogen, except oxygen, and thus act both as bases and acidifying principles under different circumstances. Yet we cannot perceive why it should be

concluded, when an acid is produced by the combination of hydrogen with chlorine or iodine, that the former should be looked upon as the principle of acidification instead of the latter. We do not recollect any acid of very marked properties, in which the acidifying principle may not be supposed to be one of the supporters of combustion, with as much reason as hydrogen. But it does not seem to be necessary to conclude that acidification is dependent upon any particular principle, any more than combustion. It may be merely the consequence of a particular mode of union, and of some hidden analogies of constitution, which have hitherto eluded investigation.

A new modification of the theory of acidification has been lately advanced, which owes its origin to Dr. Coxe of Philadelphia, was adopted to a certain degree by Davy, and has been finally matured and presented to the world by Dr. Murray. Water has always appeared essential to the constitution of the stronger acids, and they have never been obtained in what was to be considered a dry state. But it may be, that the elements of the water do not enter into the composition of the acid as water, but as oxygen and hydrogen; and the reason why dry acid cannot be obtained is, that no such thing can exist; and all the water has been separated, which is separable without the decomposition of the acid.

‘It is conceived by Dr. Murray that from the united action of the elements, viz. oxygen and hydrogen, a higher degree of acidity is acquired, than from the influence of either of them alone; and sulphur is quoted as affording a striking example. With hydrogen it forms a weak acid (*sulphuretted hydrogen*). With oxygen it also forms an acid, (*sulphurous acid*), which though of superior energy, still does not display much power. With hydrogen and oxygen, it seems to receive the acidifying influence of both, and its acid (*sulphuric acid*) is proportionally exalted.’ p. 525.

Similar views may be taken of the compounds of nitrogen and of carbon; and there seem to be strong grounds for the opinions which Dr. Murray advances. He has extended the same theory to the subject of the alkalies.

‘As hydrogen in some cases gives rise to acidity, so it may in other cases occasion alkalinity. Under this point of view

ammonia, in which oxygen cannot be detected, is a compound of which nitrogen is the base, deriving its alkaline power from hydrogen; it stands therefore in the same relation to the other alkalies, that sulphuretted hydrogen does to the acids. The fixed alkalies, potash and soda, are considered as hydrates, that is to say, as bases united chemically with a certain proportion of water; but it is obvious, says Dr. Murray, that the elements of the water may exist in combination with the base; that potash, for example, is not a compound of oxide of potassium with water, but of potassium, oxygen and hydrogen.' p. 531, 532.

As a consequence of these views, the constitution of the neutral salts must be different from what is commonly supposed.

‘Neutralization is not the saturation of acid with alkali, and the subversion of the properties of one by the opposed action of those of the other; but is the change of composition of both; and the quiescence of the elements in that proportion in which their affinities are in a state of equilibrium without any excess.’

‘All these results display more fully the extensive relations of the two elements, oxygen and hydrogen. They do not act merely in opposition, as has been imagined, but more frequently in union, producing similar effects. Hydrogen is of nearly equal importance with oxygen, and the principal details of chemistry consist in their modified action on inflammable and metallic bodies.’ p. 533.

In examining a system of chemistry of recent date, we cannot but be astonished at the accumulation of facts which has been made within the present century. Indeed, that part of the volume before us which relates to the details of the science, seems almost wholly occupied in the narration of new discoveries. As has been before observed, three substances are now classed with oxygen as supporters of combustion—chlorine, formerly the oxymuriatic acid; iodine, an entirely new substance strongly resembling chlorine; and fluorine, the supposed base of the fluoric acid, and bearing the same relation to it that chlorine does to the muriatic. To the class of simple inflammable bases, not metallic, two new substances have been

added, boron and silicon. The former combined with oxygen forms the boracic acid, whose composition was unknown in the days of Lavoisier; the latter is the base of silex, and is found to resemble more nearly this class of elements, than the earths with which it was formerly arranged. The alkalies and earths have been decomposed and proved to be metallic oxides; and a new alkali and a new earth have been added to the number of those formerly known to exist.

To enter into any account of the various compounds which have been brought to light, and can be formed from these various elements, would be at once useless and uninteresting. Yet it is worth while to state, that out of about forty, described in the present volume, at least one half are of very recent discovery, and of these the greater proportion are artificial or invented—if we may use the expression—that is, not existing in nature, but put together by the ingenuity or good fortune of the chemist.

Yet, notwithstanding all the brilliant discoveries, and the important truths which have graced the progress of modern chemistry, it is difficult to avoid regretting the overthrow of so simple and beautiful a system as that of Lavoisier. It is impossible for those of us, who have formed our ideas of the chemical operations of nature on the principles which he taught, to turn with complacency from a theory like his to a state of science so unsettled and so obscure, as modern chemistry now is. That the progress of discovery has most unquestionably overthrown the foundations of the old theory, we cannot doubt; yet it has substituted nothing in its room; and the science consists now in an immense mass of facts without any regular and consistent connexion. This, from the nature of the thing, is inevitable; and our chemists must leave to their successors the task of arrangement and generalization. They have torn down but have not built up. They have overthrown the works of Lavoisier, and yet there seems to be scarcely any thing certain in speculative chemistry, except what remains of his theory. And after all, he who desires only to understand the chemical constitution and operations of nature, will find them better explained and more easily understood upon the principles of the old school. Not that we would undervalue

the alterations and discoveries of our own times; they are a natural consequence of the advanced state of the science, and lead to its ultimate perfection. Yet they have made it less captivating to the general scholar; they have lessened the interest with which it is viewed by those not immediately engaged in its pursuits, by rendering it more complicated and more difficult to be understood, and less applicable as a whole to the explication of those phenomena of the natural world with which we are most familiar.

Chemistry has hardly received, in our own country, that attention, which its real importance, and intimate connexion with our national prospects and improvement, would seem to demand. The contributions which this science has made to the arts, the improvements it has suggested in many important manufactures, and above all the security it has given to the lives of thousands by means of the celebrated safety-lamp of Sir H. Davy, prove how much an enlightened and well directed philosophy may do to improve the condition of mankind. This consideration gives it a high claim to the attention of a nation like ours, and yet we have but few men among us, who have made it their profession to understand and teach it; and it is only to such that we can look for any important accessions of knowledge. Chemistry has become in Europe almost a separate profession; and we consider it as a most desirable event to have men of abilities and enterprize, devoted to it from inclination, who are at leisure to give themselves up wholly to its pursuit; and who are not obliged to labour in other occupations for a support, whilst the cultivation of their favourite study is only their relaxation and amusement. When we have such men among us, then and not till then shall we be able to contribute our full share to the science of the world; and to turn to the best account the various means which chemistry affords for the improvement of our own condition.

The length to which we have extended this article, as well as the particular remarks which we have had occasion to make on the character of the work under review, are sufficient proofs that we estimate it highly. We regard it, as far as it has yet been published, as one of the best introductions to the science with which we are acquainted. We look forward with interest to the publication of the second volume.

American Medical Botany, with coloured engravings. By Jacob Bigelow, M. D. Mem. Amer. Acad. Arts and Sciences, American Philosophical Society, &c. Rumford Professor, and Lecturer on Mat. Med. and Botany in Harvard University. Vol. I. Part II. Vol. II. Part I. Boston, Cummings and Hilliard, 1819.

[From the North American Review, for June, 1819.]

THE appearance of the third number of Dr. Bigelow's work has completed one-half of his present inquiries on the medical botany of this country. We have in a former number offered an analysis of the first half volume of this work, and avail ourselves of the opportunity afforded by the publication of the third, to say something of its progress, and of the character under which it has advanced. It was natural for the author, when arrived at this part of his work, to look back on what he had done, and while reviewing his progress, to say something of the prospects of his undertaking. 'It gives him pleasure, he remarks, to state, that the reception of his work, in all parts of the United States, has exceeded his anticipations; that the subscription is already more than sufficient to defray the expenses of publishing, and that its regular increase renders it probable that the whole edition will be taken up at an early period.'

There are two views under which this work may be regarded, as a specimen of *art*, and as a *scientific publication*. The engravings are executed by a peculiar method, and one which we believe is at present but little known, at least among us. In his advertisement to the second volume, the author informs us, that 'the style of engraving is wholly new in this country, and is one which has been successfully attempted only by the first artists in France.' The peculiarity consists, we understand, in this; that the engravings are printed in colours applied to the copperplate, instead of being printed in black by the usual method and afterwards coloured by the hand. The second and third numbers, and likewise all of the first except about two hundred copies, are executed in this manner; the first plates having been re-engraved to adapt them

to the new method. This art, which though not wholly new, appears in this instance to be original, is the result of a series of experiments made under the direction of the author, by Messrs. Annin and Smith, engravers in this town. Considering the difficulties which attend the process, the result appears to us very successful. The plates we think are not all equally good, yet in most of them there is a delicacy of finishing in the lights and shades, which it would require great labour of the pencil, bestowed on each copy, to produce in the common manner. There is besides in these plates an entire absence of black outlines and veins, which are found in engravings, so that the figures in this work resemble original drawings or paintings, rather than engraved copies. There is also an advantage arising from this method, that an unlimited number of copies may be produced exactly alike, and varying far less than copies separately painted by the hands of different persons, as they must necessarily be in large works executed in the old manner. In this method a sufficient quantity of colour may be prepared at once for the whole impression of a plant. When the colours are simple and not much blended, a single plate may be employed; but when they are complicated, two or more plates must be used containing different parts of the plant. In the latter case the process of impression must be performed once for every plate. A certain degree of skill, which can be acquired only from practice, is necessary in the engraver, and a dexterity of the same kind is requisite in the pressman. The work of the press is much slower than in ordinary copperplate printing, yet more rapid than the usual mode of washing in colours.

Printing in colours has been at various times attempted in England and on the continent of Europe. It has however been abandoned, we are told, on account of the difficulty and expense attending it. In France it has recently been revived with satisfactory success. The elegant work of Michaux on the forest trees is partly executed in this manner, the lines and shades being printed in colours, and the surface afterwards washed with the same. The only finished works which we know, in which the surface, outlines and shades are said to be produced together from the copperplate, are those of

M. Redouté, whose large and splendid *livraisons* on the *Liliaceous plants* and the *Roses*, are among the most perfect specimens of botanical imitation which any country has produced. It appears that these magnificent publications have employed their author since the year 1796. A late number of the *Journal des Savans*, informs us that the method of *M. Redouté*, which they consider as having produced the most perfect specimens of botanical iconography, consists in the application of various colours to the surface of one copperplate, by modes peculiar to the author, and which he proposes to make public at a future day. The reviewers object to the old method, that it is attended with an inequality in the copies and an irregularity in the light and shades; and that the black lines which do not exist in nature prevent the imitation from being faithful. From these objections they consider *Redouté's* works as exempt, at the same time that his plates have all the softness and finish of original paintings.

Although it appears that the French process is hitherto kept a secret, there can be but little doubt that the principles on which the American work is executed are virtually the same. No greater inferiority exists than is naturally to be expected in a country where the arts are in infancy. In comparing the earlier with the later specimens of the American Medical Botany, we think a regular improvement is visible, and confidently look forward to future numbers for a perfection in this method of engraving, which will be creditable to the country.

We have heard it remarked, that the new engravings in this work want the finished appearance, the strongly defined outline of other botanical drawings. In these engravings the colours themselves of the leaves, flowers, &c. constitute their outline. This want of a terminal or marginal line to leaves and flowers, of a different colour from other parts of these portions of the plant, is in exact conformity to nature, which has given them no such outline, and it must be because we have determined what plants are, from drawings, and not from nature, if we consider this naturalness of Dr. Bigelow's figures a defect instead of a beauty. The accuracy with which this mode of engraving may be made to imitate different parts

of a plant, is very apparent in the plates of *Ictodes fœtidus* and *Panax quinquefolium* in the third number.

The mechanical execution of other parts of this work deserves notice and commendation. The type is remarkable for its clearness and beauty, and no pains have been spared to render the work elegant and correct.

Regarded as scientific publications, the second and third numbers of the *American Medical Botany* sustain the character of the first. The interest of the author remains unabated, and in the numbers under notice, he furnishes novel and valuable information about the medicinal properties of a number of our plants.

BIOGRAPHY.

Memoirs of the Life of LEWIS BRUGNATELLI, M. D. Professor of Chemistry in the University of Pavia, Member of the Imperial-royal Institute of Science, Literature and Arts of Milan, &c. &c.

[From the Philosophical Magazine and Journal, for May, 1819.]

MODERN Italy has produced few men whose lives have been more useful to society, or more interesting, not merely to their own country but to the civilized world, than that of Dr. Lewis Brugnatelli; who presents an illustrious example of talents and industry attaining eminence in science, although unsupported either by personal wealth or powerful patronage. In any other country than Italy, the number and immense sale of his literary works on science would have procured him additional means of making chemical researches; but unfortunately for writers in the Italian language, a work of merit is no sooner published in any of the principal cities, than it is reprinted in all the adjoining states; and, in the commercial phrase, the market is often supplied with editions so shamefully incorrect, as to injure not merely the interest but also the honour of the original author. It is necessary to consider well the fatal consequences of this state of things, in order to appreciate more adequately the zeal and indefatigable exertions of the late chemical professor.

Dr. Lewis Brugnatelli was born in Pavia in 1761; his parents, not being in very affluent circumstances, had destined him for a mercantile life before he had received the rudiments of a literary education: observing, however, the strong bent of his mind, they afterwards thought of making him an engineer; but this study, although scientific, was little congenial to his feelings, and he immediately applied himself with the most in-

defatigable zeal to the study of medicine and chemistry; in which his progress was so rapid, notwithstanding the extreme scantiness of his means, that he not only obtained the degree of Doctor of Medicine in Pavia, 1784, but was shortly after elected Repeater of chemistry in the same university. By the death of Professor Leopoli he became pensioned Repeater in the College of Ghislieri, and in 1787 he was elected assistant to the chemical chair of Professor Leopoli, and afterwards to that also of Professor Brusati. During this interesting period he had given the most unequivocal evidence of his talents and skill both in chemistry and medicine. The science of analytical chemistry had just come into existence; curiosity and enthusiasm were awakened towards every thing that could be subjected to chemical action: a few chemical reagents had been discovered, and our juvenile professor eagerly availed himself of their aid to investigate the nature and properties of the gastric juice. His experiments were made and published the very year in which he graduated (1784), when he discovered that the gastric juice had invariably an acid character in carnivorous animals, while in herbivorous it was uniformly alkaline and putrescent. He was led to these experiments by the circumstance of Professor Carminati being engaged in making physiological researches on the gastric juice at the same period in the hospital. It was then ascertained, that the gastric juice of carnivorous animals had great curative powers when applied to foul ulcers or wounds, but that of herbivorous was destitute of this property. Professor Brugnatelli continued his researches; and combining the effects of the different kinds of gastric juice with that acid which he had also discovered in the stomach of all carnivorous birds, he succeeded in determining their solvent powers in the corrosion not only of metals but calcareous stones; and even pieces of rock-crystal and agate, introduced into the stomach, exhibited signs of its consumptive powers. These experiments were followed by an examination of the action of nitric acid on cork, in which the Professor discovered that a new and peculiar acid was developed, and which has since been called the suberic acid. At the same time he discovered a method of preparing fulminating silver, which he improved and extended to other substances; and which is

esteemed preferable to the process of Howard, being that now generally used for making fulminating balls, &c. The consequence of these discoveries led him to make new experiments on the salts (particularly nitrats) which had the property of detonating when mixed with a combustible body, and exposed to friction or a blow of a hammer; in order to demonstrate the quantity of caloric which might exist in bodies even in the solid state. He extended his ideas to the various kinds of combustion, proving the necessity of determining the difference between them; some being cold and obscure, others accompanied with the most vivid developement of caloric and light,—circumstances which must have a very great influence on the properties of a body that was united to oxygen. In the case of cold and obscure combustion, the body continues capable of presenting the detonating phænomena of caloric and light, when brought in contact with other combustible bodies; but it loses entirely this property if the caloric and light were disengaged previous to its union with oxygen. Of these facts and observations Thomson availed himself in his *System of Chemistry*, article *Combustion*, which is chiefly derived from the luminous researches and ingenious observations of the Pavian Professor. On these facts was founded the hypothesis respecting the constitution of oxygen, modifying the principles of Lavoisier, according to which many phænomena of combustion are very plausibly explained.

Among the ingenious researches and observations of Professor Brugnatelli, must be noticed, his opinion respecting the chemical action of the electric fluid, which he published so early as 1800, in his ‘*Memoir on oxyelectrics*,’ inserted in his *Annali di Chimica*, vol. xviii. In his ‘*Galvanic observations*’ published in the same work and in the *Memoirs of the Italian Institute*, he opposed decidedly the supposed formation of muriatic acid at the expense of water, as observed by Pacchiani, remarking, that this acid depended on other substances pre-existing in the water. By these and other observations he made some progress towards those discoveries which have immortalized the name of Davy; who, on his part, did not fail to cite with great care and merited approbation the previous experiments of the Pavian chemist.

Professor Brugnattelli being at Paris in 1801, and in company with Volta, he mentioned the fact, that various substances are transported by the electric fluid, in presence of the French chemists and philosophers: all of whom smiled, saying, "The thing is impossible, that an imponderable body should transport ponderable substances." So far had the Italian chemist, as usual, anticipated the knowledge of the progress of chemical discovery, even in the French capital, where his experiments and discoveries were so novel and singular, that they were boldly and thoughtlessly disbelieved, instead of being investigated, verified or disproved. The Pavian Professor, however, had previously proposed a modification or a reform of the new chemical nomenclature; and as the greater part of the nomenclaturists were then living, it was the easiest and most effectual mode of avoiding difficulties, by totally disbelieving both the chemical and literary novelties. In 1806 he read a Memoir in the hall of the University, 'On the decomposition of salts effected by electricity,' which was afterwards printed in the first volume of his *Giornale*.

To detail with sufficient accuracy his numerous discoveries in pneumatic, vegetable and animal chemistry, would greatly exceed the limits prescribed to this brief memoir: to those pursuing similar inquiries, the subjoined list of his original works, copied from the *Giornale di Fisica*, edited by his son Dr. Caspar B. may be useful. It may likewise be proper to notice here that he discovered uric acid in the excrement of silkworms, free lime in rhubarb, and carbonate of lime in the urinary calculi of hogs, and more recently in those of men. His numerous experiments and researches appear in a posthumous 'Memoir on urinary calculi,' which is just published, and which abounds in new facts and observations, the result of great industry and extensive knowledge, derived from a vast collection of calculi, designs of which accompany the work. Among his researches in vegetable chemistry should be recorded his Experiments on Coffee-berries, which, being steeped some time in a solution of soda, displayed a beautiful emerald green; this colour, the same as occurs in ammoniure of copper, is dissipated in close receivers, but immediately reappears when brought in contact with atmospheric air. He also discovered

several new sympathetic inks, some hygrometric colours, reagents to detect poisonous substances; and greatly improved many pharmaceutical and chemical processes, introduced various new amalgams and paints, and obtained a very pure gum from the variegated aloes and various other vegetable products. His electric and galvanic experiments were equally numerous: and the curious fact of carbon becoming capable, by means of galvanism, of being oxygenated and hydrogenated, and when in this state a powerful electric, may contribute to facilitate further experiments of the like nature. The medical labours of professor Brugnatelli would have given him celebrity, had his chemical fame been less conspicuous. His experiments with chlorine in the cure of hydrophobia are too recent to require further notice; but whatever may be the final effects of this medicine, either in curing or mitigating a hitherto incurable disease, the merit of Brugnatelli in recommending it to the public must ever remain unimpaired. In such a calamity, every truly scientific medical practitioner will gladly avail himself of a medicine, which presents even the slightest hope of arresting the hand of Death, and which is so easily procured as to deprive either indolence or ignorance of a pretext for not promptly administering it. The observations and statements of the Pavian Professor have been translated into almost all the European languages; and should any obstinate or wilfully incredulous practitioner omit its application, he will necessarily expose himself to the censure of friends.

Finally, Brugnatelli was appointed professor of general chemistry applied to the arts, in the University of his native city, Pavia, in 1796; and he filled this chair with equal honour to himself and advantage to the numerous students from all parts of Italy and the Levant who attended his lectures, till his death on the 24th of October 1818, in his fifty-eighth year. The following list of his published writings is principally taken from the catalogue printed in the *Giornale* edited by his son. Original works:—‘Elements of chemistry;’ four editions of this work have been sanctioned by the author, how many have been pirated it is impossible to tell. ‘A General Pharmacopœia;’ of this, five editions have been printed, and it has been translated into other languages. ‘Materia Medica,’ a supplement to the

preceding, in one volume. The periodical works which he edited were:—*Biblioteca Fisica d' Europa*, from 1788—91, 20 volumes. *Giornale Fisico Medico*, afterwards continued under the title of *Avanzamenti della Medicina e Fisica*, 1792—96, 20 volumes. *Annali di Chimica*. 1790—1805. *Commentari Medici*, edited in company with Biera, 1797, one volume. *Giornale di Fisica, Chimica e Storia Naturale*, 1808—18; the first eight volumes were edited by Brugnatelli alone, the remainder in company with Brunacci, Configliachi, and his son.

His detached memoirs and papers must be enumerated in their chronological order. In 1784 appeared his 'Letter on the means of preserving various insects, and chemical analysis of the gastric juice,' in the *Oposculi scelti* of Milan, vol 7. In 1785, 'Letter on the solvent power of the gastric juice of certain animals,' *ib.* vol. 8; and 'On the peculiar properties of vitriol of iron,' published in *Crell's Annals*. In 1786, 'Memoir on the nature of cork,' *Opus. scelt.* Milan, vol. 9. In 1787, 'On the action of turnsole upon animal matter' 'On the sediment of urine, On the corruption of animal matter in different kinds of acid, and Experiments on the constituent parts of alcohol, of gall,' &c. all published in *Crell's Annals*. In 1788, 'Fructification of the rose, and analysis of the saliva,' *Rozier's Journal*, vol. 33; 'New sympathetic inks, method of restoring ancient writings, and discoveries respecting vegetable substances,' *Biblioteca*, vols. 3 and 4. In 1789, 'New method of obtaining acid from concentrated vinegar,' *ib.* vol. 10. In 1790, 'Method of rendering paper and ink indestructible by fire, new mode of bleaching wax,' *ib.* vols. 14 and 17. 'Singular property of certain substances to move themselves upon water,' *Ann.* vols. 1 and 22; 'On oxygenated muriatic acid used as a photometer, and easy mode of discovering nitrous in vitriolic acid,' *ib.* In 1791—2 and—3 appeared 'A new mode of preserving and concentrating citric acid, a new neutral salt, observations on some insects, and chemical reagents for the use of travelling naturalists,' *ib.* vol. 4. In 1794, 'Easy mode of impregnating water with the acidulous carbonat of potash, on the perennial heat of the tepid water of St. Pellegrino, and chemical analysis of the vegeto-mineral mire of Trescore,' *ib.* vols. 5 and 6. In 1795, 'Letter on animal electricity, on caloric and light, medi-

cal observations, and proposals for reforming the new nomenclature,' *ib.* vols. 7, 8, 9, 10, and 13. This latter work excited considerable attention to the chemical and literary labours of the Pavian professor: the nomenclature which he proposed has been generally adopted by Italian chemists, and it is perhaps well suited to the genius of the language. Although subsequent discoveries have not sanctioned the principles which it favours, it has nevertheless been extremely useful in Italy, in a country where numbers read and speak of chemistry and chemical subjects without any practical knowledge; where there are very few practical, but many theoretical and critical chemists, who, being amused with literary subtleties and refinement of terms, have disseminated a taste for chemical studies, which may ultimately lead to practical experience, and consequently useful discoveries. In 1796 and two following years, he published his 'Researches on combustion, the action of medicines on the animal body, on phosphorus, and fulminating bodies, saccharic acid considered as a reagent, convenient apparatus for making carbonated and other mineral waters, on fulminating gold, on the difference between oxygen and termoxygen, process for making mosaic gold, on urinary calculi, description of a compound still to obtain brandy and alcohol at the same time; on ammoniure of cobalt, and an acid in zaffre; method of obtaining crystallized oxymuriate of lead and of calomel without corrosive sublimate; on ethers, albumen, ammoniures of mercury and zinc,' *Ann.* vols. 10—22. In 1800, he again published 'A table of the modern chemical nomenclature,' which occasioned some controversy: but his principal exertions were directed to galvanic experiments during this and the two following years. Among the papers, however, which he published during this period, may be mentioned his 'Observations on vesicular vapours suspended in air at the freezing temperature, on the conversion of fixed oils into wax, sensibility of plants, a detonating oxymuriate of lead, and on the phosphorism of animal bodies.' In the Memoirs of the Italian Institute for 1806, appeared his 'Observations on the identity of some new characters of carbon with those of the metals,' which have since been so amply illustrated. Many other memoirs and translations of chemical and medical works issued from his pen; and

his countrymen now begin to appreciate more justly his merits as a philosopher, when they can no longer enjoy his amiable character as a man. Fortunately for them and the friends of science, his son and successor pursues with success the noble career of his father; and Brugnatelli's Journal, almost the only scientific periodical work at present published in Italy, may continue to be the vehicle of new discoveries in the arts and sciences, to enlighten and instruct some of the most ingenious, friendly and good-natured people in the world.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

METEOROLOGICAL OBSERVATIONS.

STATE of the weather at Philadelphia during the last six months of 1819.

JULY.

Thermometer—Lowest at 8 A. M. 67. 7th day of the month.

Highest at 3 P. M. 92. 31st day.

Mean, 78.

Winds chiefly south-west—heavy rain 26th—An hot and dry month. The season uncommonly hot and dry in Europe. A comet appeared on the 4th inst. in the north-west, but was seen for a short time. Some cases of measles in this and last month—June 28, several cases of malignant fever, which continued in this and next month, but did not become epidemic—the whole number affected did not exceed twenty-two persons—the disease was unusually mortal.

AUGUST.

Thermometer—Lowest at 8 A. M. 63. 27th day of the month.

Highest at 3 P. M. 94. 1st day.

Mean, 75.

Winds variable—between east and west—little rain—and not much thunder—Crops of winter grain good—Indian corn, a moderate crop—Peaches and melons abundant, and good—Apples scarce—Vegetables good and plenty—Whooping cough and Cholera infantum prevalent—Measles continue—The Plague rages in Africa.

SEPTEMBER.

Thermometer—Lowest at 8 A. M. 54. 21st day of the month.

Highest at 3 P. M. 88. 3d day.

Mean, 70.

Winds very variable—Some refreshing showers—A very severe hurricane in the West Indies, on the 20th and 21st of the month—Malignant fever at Boston and New-York, but did not spread generally in those cities; although the number

of deaths was very considerable—It was epidemic at Baltimore, Charleston, Mobile, Orleans, Natches and Bermuda—The measles continue to spread at Philadelphia—The malignant fever prevailed extensively, in this, and the two succeeding months, at Cadiz, and in its vicinity.

OCTOBER.

Thermometer—Lowest at 8 A. M. 38. 26th day of the month.

Highest at 3 P. M. 76. 7th day.

Mean, 55.

Chiefly westerly winds—mostly dry weather—On the 12th of the month an aurora borealis, with a beautiful bow, leading from the eastern to the western horizon, at 8 o'clock P. M.—Measles spreading.

NOVEMBER.

Thermometer—Lowest at 8 A. M. 29. 30th day of the month.

Highest at 3 P. M. 67. 12th day.

Mean, 48.

Westerly winds—heavy rain on the 16th—generally dry—Measles continue to spread.

DECEMBER.

Thermometer—Lowest at 8 A. M. 21. 30th day of the month.

Highest at 3 P. M. 50. 7th and 9th days.

Mean, 40.

Winds westerly—little rain—several small snows—and an heavy fall of snow on the 30th—The whole season has been healthy—Small-pox but little heard of—Measles have now become epidemic—a mild kind.

DISPENSARIES.

By the statements published, it appears, that the number of patients under their care have been:

Philadelphia Dispensary.

From December 1st 1818, to December 1st 1819,	2,294
Of these the number Cured is	- - - 2,022
Relieved,	- - - 82
Dead,	- - - 69
Irregular or uncertain,	- 31
Removed,	- - - 34
Remaining under care,	- 56—2,294

Southern Dispensary.

From December 1st 1818 to December 1st 1819,	1,642
Of these the number Cured is - - -	1,484
Relieved, - - -	73
Dead, - - -	33
Removed, - - -	11
Remaining under care,	41—1,642

Northern Dispensary.

From December 1, 1818 to December 31, 1819,	745
Of these the number Cured is - - -	583
Relieved, - - -	42
Removed, - - -	32
Irregular, - - -	17
Dead, - - -	44
Remaining under care, -	27—745

Making a total of four thousand six hundred and eighty-one.



New French mode of discovering Ulcers in the Lungs. Extracted by Mr. GRAY, from l'Analyse des Travaux de l'Académie Royale des Sciences pendant l'Année 1818. Par M. LE CHEVALIER CUVIER.

[From the London Medical Repository, for August, 1819.]

IN a climate so variable as England, where, according to common opinion, pulmonary complaints are more usual than in other countries, and where so very large a proportion of our youth are cut off yearly by consumptions, every means of improvement in respect to the diagnosis, and to the ascertaining of the progress of ulceration in the lungs, must be of great value to the profession.

Dr. Laennec, a physician of Paris, has presented to the Royal Academy of Sciences, a memoir upon the mode of exploring the diseases of the thorax by means of the communication of sound. For this purpose he sometimes employs a solid cylinder; at other times a tube, the sides of which are very thick; and at others, a tube spread out at one end like a funnel. One end of these instruments is applied to different

parts of the thorax, and the ear of the physician applied to the other.

The thick sided tube, or, in other words, the cylinder pierced along its axis by a narrow tube, being applied to the chest of a person who is talking or singing, and who is in good health, only communicates to the ear of the physician a sort of humming noise, more or less distinct; but if the person has an ulcer in his lungs, a singular phenomenon occurs. The talking or singing is no longer heard by the ear which is not applied to the tube, but the sound is conveyed entirely to the physician by the tube. The same phenomena occur when the tube is applied to the trachea or larynx of a healthy person.

By means of these instruments the movements occurring in respiration, and the beating of the heart, may be distinctly heard; so that their regularity, or disordered motion, may be estimated with facility.

The Royal Society of London.

[From the London Medical and Physical Journal, for August, 1819.]

A paper by Dr. FERGUSON was read, January 18th, 1819, *On the poisonous fishes of the Caribbee Islands*. The author endeavoured to prove, that, in all the larger fishes of prey, the poisonous quality was a rare and accidental occurrence; and that it was found to be present only at a certain season in the year, in one or two of the smaller species of fish, more particularly in the *yellow-billed sprat*, the *sardine doré* of the French, and *clupea thryssa* of naturalists; whence he inferred, that the larger voracious fishes, such as the *baracosta*, *perca major* of naturalists, &c. became poisonous only at the times they had recently been feeding on the smaller poisonous prey. The notion of these being made poisonous from lying on copper-banks, or their eating the stinging blubbers, the *medusæ* and *holothuriæ*, was refuted. In regard to the tests, it was shown that none could be depended on; that nothing whatever could be discovered from inspection of the fish; that the test of boiling a piece of silver with the suspected fish, proved nothing, whatever might be its actual quality; that, so far from there being any marks of disease in the viscera, or other parts,

of poisonous fishes, they were generally found to be in the best state in every respect.

The poison of the yellow-billed sprat was supposed to exist in the animal at certain seasons of the year, and not to be occasioned by its having fed on any undiscovered local marine poison, from the circumstance of the other smaller fishes of the same genus, that were found in the same places, never partaking of the poisonous nature; and from the poison of the fish being more potent and deadly than any other known, or even supposed, article of food could be likely to communicate.

With respect to remedies and antidotes, the efficacy of sugar was alone established as deserving of credit. Wines, spirits, and the condiments used at table, were believed to have obtained occasional credit only from their being used in slight cases of the poison, as would most likely have terminated in a favourable manner without any remedy. As a precaution in all cases of suspicious fish of the larger species, the cleaning them out as soon as caught was recommended as a useful and proper one, to prevent the carcass being further tainted by the lodgment of any poisonous matter, such as that of the yellow-billed sprat, recently swallowed; though it was shown, at the same time, that the doing so, and even salting the fish afterwards, could not, in any instance, remove the poisonous impregnation so communicated to these voracious creatures; whose powers of assimilation, from the shortness of the intestines and great size of the liver, must be supposed to be infinitely quicker than what takes place amongst terrestrial animals. It was useful also in another way, by furnishing the material of the only criterion hitherto discovered for detecting the poison, which was shown to be that of giving a portion of the liver or offal to some inferior animal, such as a cat, a duck, or a pig, and ascertaining its effects upon them before making use of the fish.

PYROLIGNEOUS ACID.

*Extract of a Letter from M. G. C. at Paris, to Professor
VAN MONS.*

“ A DISCOVERY of the greatest importance engages at this moment the attention of the scientific world. A. M. Monge has

discovered, that the pyroligneous acid obtained from the distillation of wood, has the property of preventing the decomposition and putrefaction of animal substances. It is sufficient to plunge meat for a few moments into this acid, even slightly empyreumatic, to preserve it as long as you please. Cutlets, kidneys, liver, rabbits, which were thus prepared as far back as the month of July last, are now as fresh as if they had been just procured from the market. I have seen carcasses washed three weeks ago with pyroligneous acid, in which there is as yet no sign of decomposition. Putrefaction not only stops, but it even retrogrades. Jakes exhaling infection cease to do so as soon as you pour upon them the pyroligneous acid. You may judge how many important applications may be made of this process. Navigation, medicine, unwholesome manufactories, will derive incalculable advantages from it. This explains why meat merely dried in a stove does not keep, while that which is smoked becomes unalterable. We have here an explanation of the theory of hams, of the beef of Hamburgh, of smoked tongues, sausages, red herrings, of wood smoked to preserve it from worms, &c. &c."

Dr. Jorg, Professor at Leipsic, has since made many successful experiments of the same nature. He has entirely recovered several anatomical preparations from incipient corruption, by pouring this acid over them. With the oil which is produced from wood by distillation in the dry manner, he has moistened pieces of flesh already advanced in decay; and, notwithstanding the heat of the weather, soon made them as dry and firm as flesh can be rendered by being smoked in the smoking-room. All traces of corruption vanish at once when the *vinegar of wood*, or the *oil of wood*, is applied to the meat with a brush. The Professor has also begun to prepare mummies of animals, and has no doubt of success. He promises great advantages to anatomy, domestic æconomy, and even to medicine, from this discovery; for the remedy seems very fit to be applied internally and externally in many disorders; and intends to publish the result of his further experiments.

Philosophical Magazine and Journal, for July, 1819.

Polyphagism.

ALL the polyphagists whose wonderful deeds are recorded in history, are superseded by the famous TARRARE, who was known to all Paris, and who died at Versailles about twenty years since, at the age of twenty-six years.

M. le Baron PERCY, who saw Tarrare, and who made some investigations respecting this singular personage, has given us the history of him, in a very curious memoir on Polyphagy: it is from this memoir that I shall extract the particulars I am about to relate of Tarrare. Tarrare has renewed amongst us the fable of ERISICHTON, who, according to OVID, devoured at one meal what might have sufficed for a whole city, or a whole nation.

—quod urbibus esse,
Quodque satis poterat populo.

At seventeen years of age, Tarrare weighed only one hundred pounds, and was already able to eat, in twenty-four hours, a quarter of a bullock of that weight. Having left his parents when very young, (he was of the environs of Lyons) sometimes begging, sometimes stealing, to obtain subsistence, he attached himself to one of the shows on our boulevards, where we see exhibit themselves, in turn, Gill, Harlequin, and Punchinello. One time, on the stage, he defied the public to satiate him, and ate in a few minutes a panier-full of apples, furnished by one of the spectators; he swallowed flints, corks, and all that was presented to him. At the commencement of the war Tarrare entered into a batallion; he served all the young men in easy circumstances in the company, did all their jobs for them, and ate up the rations they left for him. Famine nevertheless gained upon him; he fell sick, and was taken to the military hospital at Soultz. On the day of his entry he received a quadruple allowance; he devoured the food refused by the other patients, and the scraps about the kitchen; but his hunger could not thus be appeased. He got into the apothecary's room, and ate there the poultices, and every thing he could seize. "Let a person imagine," says M. Percy, "all that domestic and wild animals, the most filthy and ravenous, are

capable of devouring, and they may form some idea of the appetite, as well as of the wants of Tarrare." He would eat dogs and cats. One day, in the presence of the chief physician of the army, Dr. LORENCE, he seized by the neck and paws a large living cat, tore open its belly with his teeth, sucked its blood, and devoured it, leaving no part of it but the bare skeleton: half an hour afterwards he threw up the hairs of the cat, just as birds of prey, and other carnivorous animals, do. Tarrare liked the flesh of serpents; he managed them familiarly, and ate alive the largest snakes (*couleuvres*) without leaving any part of them. He swallowed a large eel alive without chewing it, but we thought we perceived him crush its head between his teeth. He ate, in a few instants, the dinner prepared for fifteen German labourers: this repast was composed of four bowls of curdled milk, and two enormous hard puddings. After this the belly of Tarrare, commonly lank and wrinkled, was distended like a balloon: he went away, and slept until the next day, and was not incommoded by it. M. COMVILLE, the surgeon-major of the hospital where Tarrare then was, made him swallow a wooden case, enclosing a sheet of white paper: he voided it the following day by the anus, and the paper was uninjured. The general-in-chief had him brought before him; and, after having devoured in his presence nearly thirty pounds of raw liver and lights, Tarrare again swallowed the wooden case, in which was placed a letter to a French officer, who was a prisoner to the enemy. Tarrare set out, was taken, flogged, imprisoned; voided the wooden case, which he had retained thirty hours, and had the address to swallow it again, to conceal the knowledge of its contents from the enemy. They tried to cure him of this insatiable hunger, by the use of acids, preparations of opium, and pills of tobacco; but nothing diminished his appetite and his gluttony. He went about the slaughter-houses and bye-places, to dispute with dogs and wolves the most disgusting aliments. The servants of the hospital surprised him drinking the blood of patients who had been bled, and in the dead-room devouring the bodies. A child fourteen months old disappeared suddenly; fearful suspicions fell on Tarrare; they drove him from the hospital. M. Percy lost sight of him for four years: at the

end of this time he saw Tarrare at the civil hospital at Versailles, where he was perishing in a tabid state. This disease had put a stop to his gluttonous appetite. He at length died in a state of consumption, and worn out by a purulent and fetid diarrhœa, which announced a general suppuration of the viscera of the abdominal cavity. His body, as soon as he was dead, became a prey to an horrible corruption. The entrails were putrefied, confounded together, and immersed in pus: the liver was excessively large, void of consistence, and in a putrescent state; the gall-bladder was of considerable magnitude; the stomach, in a lax state, and, having ulcerated patches dispersed about it, covered almost the whole of the abdominal region. The stench of the body was so insupportable, that M. TESSIER, chief surgeon of the hospital, could not carry his investigation to any further extent.

Tarrare was of a middle-sized stature; his habit of body was weak and slender; he was not of a ferocious spirit; his look was timid; the little hair he had preserved, although very young, was very fair, and extremely fine. His cheeks were sallow, and furrowed by long and deep wrinkles: on distending them, he could hold in them as many as a dozen eggs or apples. His mouth was very large; he had hardly any lips; he had all his teeth, the molares were much worn away, and the colour of their enamel streaked like marble; the space between the jaws, when they were fully separated, measured about four inches: in this state, with the head inclined backwards, the mouth and œsophagus formed a rectilinear canal, into which a cylinder of a foot in circumference could be introduced without touching the palate. Tarrare, says M. Percy, was constantly covered with sweat, and from his body, always burning hot, a vapour arose, sensible to the sight, and still more so to the smell. He often stank to such a degree, that he could not be endured within the distance of twenty paces. He was subject to a flux from the bowels, and his dejections were fetid beyond all conception. When he had not eaten copiously within a short time, the skin of his belly would wrap almost around his body. When he was well satiated with food, the vapour from his body increased, his cheeks and his eyes became of a vivid red; a brutal somnolence, and a sort of hebi-

tude came over him while he digested. He was in this state troubled with noisy belchings, and made, in moving his jaw, some motions like those of deglutition. M. Percy never saw in him any signs of rumination. The young Tarrare was almost devoid of force and of ideas. When he had eaten to a moderate extent, and his hunger only appeased, he was quick and active; he was heavy and sleepy only when he had eaten to excess.—*London Medical and Physical Journal, September, 1819.*

Hyposulphuric Acid.

A new acid has been recently discovered by MM. Gay Lussac and Welter, which they have called *Hyposulphuric Acid*, and an account of which was communicated to the Institute of France, on the 5th of April 1819. They obtained it by passing a current of sulphurous acid gas, over a solution of peroxide of manganese in water; then filtering and pouring into the liquor, a certain quantity of barytes, and causing a current of carbonic acid gas to pass over it, if there is an excess of this; then by pouring upon it sulphuric acid, the barytes is thrown down, and the new acid is obtained, which is dried under the receiver of an air-pump, by sulphuric acid. The greater number of the salts which it forms with earthy or metallic bases, are soluble, and crystallize. The hyposulphates of barytes and lime are inalterable in air; and the suberic acid and chlorine do not decompose the hyposulphate of barytes. This new acid is composed of two proportions of sulphur, and five of oxygen.—*Edinburgh Philosophical Journal, June, 1819.*

Raiz Preta, or Black Emetic Root.

The natives in the interior of Brazil use the infusion of the root of a plant, somewhat resembling ipecacuanha, with great effect, in the cure of dropsy, and in destroying the dangerous effects produced by the poison of serpents. When taken, it produces vomiting, and afterwards acts most powerfully on the urinary organs, occasioning for five or six days an extraordinary flow of urine. One dose is said to be sufficient for the cure of the bite of serpents, but many are required for the removal of dropsy.—*Edinburgh Philosophical Journal, June, 1819.*

Scientific Travellers in Brazil.

It is probably not generally known that at this moment scientific travellers are traversing all parts of Brazil, under the protection of the Portuguese, and at the expense of the Austrian, Bavarian and Tuscan Governments. On the part of Austria, the following are employed: 1. Professor Mikan for natural history in general, and botany in particular: 2. Dr. Pohl as mineralogist: 3. M. Natterer for Zoology: 4. M. Schott as gardener: 5. M. Socher as huntsman: 6. M. Ender as landscape-painter. 7. M. Buchberger as botanical painter, and M. Frick as natural history painter. On the part of Bavaria, 1. Dr. Spix as zoologist, and 2. Professor Martinus as botanist. On the part of the Grand Duke of Tuscany, Dr. Radi as Naturalist.—*Edinburgh Philosophical Journal*, June, 1819.

AMERICAN PHILOSOPHICAL SOCIETY

At an election of officers of the Society, held at their Hall on the 7th January, 1820, the following persons were chosen.

President—Robert Patterson.

Vice Presidents—William Tilghman.—Peter S. Dupon-
ceau.—Zaccheus Collins.

Secretaries—Thomas C. James.—R. M. Patterson.—Robert Walsh, Jr.—George Ord.

Counsellors for three years—James Gibson.—Nathaniel Chapman.—Robert Hare.—William Hembel.

Curators—Joseph Cloud.—Thomas T. Hewson.—Reuben Haines.

Treasurer—John Vaughan.

National Pharmacopœia.

The General Convention, for the formation of a National Pharmacopœia, met at the City of Washington, January 1st, 1820.

LIST OF AMERICAN PUBLICATIONS.

Chemistry.

A System of Chemistry for the use of Students of Medicine. By Franklin Bache, M. D. Member of the Academy of Natural Sciences of Philadelphia.

The Elements of Experimental Chemistry, by William Henry, M. D. F. R. S. &c. The first American from the eighth London Edition, comprehending all the recent Discoveries, together with an Account of Dr. Wollaston's Scale of Chemical Equivalents. Also, a Substitute for Woulfe's or Nooth's Apparatus; and a New Theory of Galvanism, by Robert Hare, M. D. &c. in two volumes.

Medicine.

Statement of Improvements in the Theory and Practice of Medicine. By Thomas Ewell, M. D.

A Materia Medica of the United States, systematically arranged. By William Zollickoffer, M. D.

Natural History.

An Analysis of the Mineral Waters of Saratoga and Ballston, with Observations on the geology and mineralogy of the surrounding country. By John H. Steel, M. D. Second Edit. improved.

Outlines of Botany, taken chiefly from Smith's Introduction; containing an Explanation of Botanical Terms, and an Illustration of the System of Linnæus. Also some Account of Natural Orders, and the Anatomy and Physiology of Vegetables. Illustrated by Engravings. For the use of schools and students. By Dr. John Locke, Lecturer on Botany. 12mo. pp. 162.

American Medical Botany, with coloured engravings. Royal 8vo. Vol. II. Part II. By Jacob Bigelow, M. D. Rumford Professor, and Lecturer on Materia Medica in Harvard University.

LIST OF RECENT EUROPEAN PUBLICATIONS.

Medicine, Anatomy and Surgery.

Synopsis Zo-nosologica: by Thomas Parkinson, M. D. 12mo.

Illustrations of the Power of Emetic Tartar in the Cure of Fevers, Inflammation and Asthma, and in preventing Consumption and Apoplexy; by William Balfour, M. D. 8vo.

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THE
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VOL. X.

APRIL, 1820.

No. II.

SELECTED PAPERS.

Some account of a case of Obstinate Vomiting, in which an attempt was made to Prolong Life, by the injection of blood into the Veins. By JAMES BLUNDELL, M. D. Lecturer in conjunction with Dr. Haighton, on Physiology and Midwifery, at Guy's Hospital.

[From the Medico-Chirurgical Transactions, Vol. X.]

IN a former paper, which was read before the Medical Chirurgical Society in the spring of the present year, (1818) I ventured, on the authority of the experiments there related, to recommend, in cases of desperate inanition, the injection of blood by the syringe. Since these experiments were published, the operation has been already once performed; and as a narration of the circumstances may, perhaps, be of service to some, who, at this moment, may stand in need of the remedy, I hasten to lay them before the Society.

A poor fellow,* of the name of Brazier, between thirty and

* Should this history appear prolix, the Society will have the goodness to remember, that in our total ignorance of the operation, every fact becomes important.

Audi!

Nulla unquam de vita hominum cunctatio longa est.

Human life is at stake; and surely the infirm may, under such circumstances, reasonably exact from the profession, those minute investigations for their safety, which the sternest of the satirists has vindicated to a slave.

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No. 38.

forty years of age, lately a patient in Guy's Hospital, was attacked with disease about the stomach; which, as subsequent dissection proved, depended upon a scirrhusity of the pylorus. It would be impertinent to my present purpose to enter into a detailed account of the various symptoms of his long illness; it may be remarked, however, that for the last few weeks, his bowels were seldom open without the use of injections, and that during the last three or four months, he had vomited the greater part of his food. The region of the stomach was frequently examined, but though he was emaciated in a high degree, neither tenderness, nor enlargement, nor hardness could be distinguished; he had no pain there, and there was nothing in the appearance of the matter vomited, which indicated ulceration: so that on the whole there was some little ground for hoping that the symptoms might, perhaps, not arise from a scirrhusity of the pylorus.

When I saw this man at the request of Dr. Chalmers, under whose care he was, the defect of sanguification had so completely exhausted him, that his dissolution was hourly expected. The veins of the limbs, I mean their trunks, were evidently shrunk; the pulse was small and feeble, and very compressible, and so indistinct, that it could not be numbered without some difficulty; the vascular system seemed nearly empty. With these marks of inanition, the other symptoms corresponded. The temperature of the limbs was falling, and the mind sinking into a state of insensibility; the muscles were become so feeble that he spoke in whispers, and found a difficulty even in stirring his limbs; and his whole person, the limbs and face especially, was so excessively emaciated, that when he lay in the bed, so that only the face and arms were exposed, he really reminded one of an animated human skeleton, covered merely by the skin. I am aware that this figure may appear a little too fanciful, but it certainly conveys no exaggerated idea of the appearance which it is designed to illustrate. To these remarks I may add, that the complexion was slightly jaundiced, and that the skin, on various parts of the limbs especially, was discoloured, with mottled patches of a livid blue tint, which seemed rather to arise from a gather-

ing of blood in the minuter veins, than from actual extravasation.

When it was first proposed to me by Dr. Cholmely, that the injection of blood should be tried in this instance, as the "only and doubtful" remedy, I felt considerable hesitation. The case was every way unfavourable, at least to the splendid success of the operation; and I could not but think it unwise, by an adventurous attempt to prolong the life of a solitary individual, to risk the character of a remedy, which, if adopted into practice, would hereafter, in all probability, preserve the lives of numbers. On seeing the patient, however, my reluctance presently gave way; his truly helpless and hopeless appearance was such as might have moved compassion, even in those who are most familiar with disease. He was evidently at the point of death. Transfusion alone could give him a chance of life. He was himself willing that the attempt should be made. Even if the operation should fail, it would probably disclose facts which might be of advantage to others. These were weighty considerations, and we determined to operate.*

For this purpose, about an inch of the right cephalic vein was laid bare, a little above the elbow, for the vessels were too much contracted to admit of the operation below it, and a longitudinal incision, about a line in length, was made with the lancet. Some gentlemen present undertaking to supply a few ounces of blood, about an ounce and a half was taken up by the syringe, and immediately infused into the vein in a gradual stream. This operation was repeated ten times, so that between twelve and fourteen ounces of blood were introduced, in this manner, in the course of thirty or forty minutes.

No very obvious changes, either morbid or of a salutary nature, made their appearance during the operation. The brain, nerves, and muscles, remained undisturbed; the respiration continued unaltered; the temperature of the body scarcely rose; and even the pulse, with the exception of a slight increase in its size, and a dubious variation of three or four

* Dr. Cholmely, Dr. Back, Dr. Wright, Mr. James South, Mr. Callaway, Mr. John South, Mr. Thomas Cox, Mr. Pollard, and several other gentlemen, were present at the operation.

beats in a minute, underwent no obvious change. It should be observed, however, that the livid discoloration of the hands, already described, gave way to a more healthy complexion; the same change, though unattended to, had probably taken place on other parts of the skin. In reply to repeated inquiries, the patient himself declared, that he perceived no unusual sensation whatever; and at the close of the operation, when speaking doubtfully of his improvement, he expressed himself in a more audible whisper than he had made use of before.

In performing the injections, some little niceties were attended to, which it may not be improper to notice. The different portions of blood were not injected in immediate succession, but at irregular intervals of five or six minutes, so as to give time for each portion to be distributed over the vascular system, before a fresh supply was poured in. In one instance, however, two measures, in another three were thrown in, at intervals of a few seconds only, in such a manner, that from three to five ounces of blood were infused, in the course of two or three minutes, yet without occasioning any obvious derangement.

To facilitate the operation, the vein was laid bare, and a probe was passed beneath it at the under extremity. As this was a first attempt, it was expected that various embarrassing circumstances might occur; and it was therefore deemed a prudent, though perhaps not altogether a necessary precaution, to obviate those, at least, which might arise from the concealment of the vein.

The little pipe, easily introduced, was secured in the vessel without the assistance of a ligature, merely by the pressure of the finger; and in order to expel the air, it was, previously to its insertion, filled with water, and retained there, on familiar principles, by placing the tip of the finger over the superior orifice.*

The syringe and tubule formed together the whole of the apparatus; and the nozzle of the syringe, sliding readily over the smooth extremity of the tube, they could be separated or

* This piece of sleight was suggested by Mr. Henry Cline, and answered so well, on repeated trials, that I think it worth notice. The exclusion of air from the apparatus is important.

united without any difficulty. This greatly simplified the operation; for the blood was taken up from the cup, and poured into the vein, with as much ease and much in the same manner too, as the anatomist infuses his injection. Two minutes scarcely elapsed during this transfer of the blood from one arm to the other.

It is scarcely necessary to add, that the syringe was made warm;* that the apparatus was air-tight; and that the greatest care was taken, that none of the air should make its way into the veins by finding a lodgment in the syringe or pipe.

The performance of the whole operation was materially assisted by Mr. Henry Cline; and though I am conscious that his talents are too well appreciated by the profession, to require any eulogy from me, I cannot forbear expressing the pleasure which I feel on this occasion, in associating my name with his. To his instructions I stand indebted for some of my earliest surgical information, and in him I view with respect, the union of an extensive knowledge of established surgery, with that chastened and well balanced spirit of caution and enterprise, which is of all others the best fitted to improve it. I could enlarge, but I forbear; the language of panegyric would be here misplaced. What I have said he will pardon, as the well earned tribute of unfeigned esteem, as far removed from the selfishness, as it is from the satire of adulatory commendation.

Although the operation, which was performed two or three hours after mid-day, produced very little effect at the time, in the course of the evening, the patient experienced a very salutary change from it. His body became warmer; his respiration remained regular; and his pulse, which by this time had acquired nearly double its former size, beat with great regularity about eighty-eight times in a minute. This was its number before the operation was performed. While I was making these observations, I was very well pleased to hear one of the hospital attendants pointing out a reddening of the extremity

* Is not the blood animated? And if so, would not a cool apparatus be preferable, as tending less to exhaust the vital principle? Blood in a cup coagulates more speedily at a higher than at a low temperature. Experiment must decide this question.

of the nose, and the increasing ruddiness of the lips, pallid and bloodless before, as well as the greater alacrity which our patient manifested, when he attempted to stir his limbs. When asked respecting his feelings, the poor fellow himself replied, "I am better," "much better," "less fainty;" and these words were certainly pronounced with a firmer utterance, and in a louder whisper, than I had heard him use before.

All these favourable symptoms continued during the night, and the greater part of the next, the second day; indeed at eight in the morning, he thought himself stronger than on the evening preceding. At this time his limbs, as well as the trunk, were remarkably, though perhaps not preternaturally warm; he passed a stool without an injection, and felt a degree of appetite, which he had not experienced for two or three weeks before; for he pressed his attendants to supply him with a little food, and drank, at intervals, about half a pint of porter. On the whole, the symptoms seemed to indicate a slight degree of excitement. Probably this excitement was analogous to that, which arises from taking food after long-continued fasting; the blood irritating the empty vessels, on much the same principle, as the aliment does the famished stomach.

It was not till the evening of this day, the 27th of September, that he began to droop; but he sank so rapidly in the course of the night, that, on the following morning, he seemed reduced to as low an ebb as before the operation. As the day, the third, went forward, he passed an involuntary stool, and suffered a recurrence of his retchings. At nine in the evening, his extremities were become cool; his pulse was disposed to intermit, and his mind, perhaps, to waver; and these symptoms gradually increasing upon him, he died at eleven o'clock, about fifty-six hours after the injection, apparently exhausted from inanition.

It deserves particular remark, that although all the marks of exhaustion, which had preceded the operation, recurred on this day; not a single additional symptom made its appearance, with the exception of a sort of white exudation, observed on the skin of the face, and giving it the appearance of having been dusted with a few grains of coarse powder. This efflorescence, seemingly emitted with the perspiration, was in all

probability of a saline nature; but owing to some misunderstanding on the part of the attendants, none of it was preserved for chemical examination.

On the morning after death, the body was examined by Mr. Callaway. From this inspection, it appeared that the *pylorus* was really scirrhus, together with the upper part of the duodenum, and that this indurated mass made a slight pressure upon the gall-ducts. Here too the passage for the food was contracted, and its inner surface irregular, though it did not appear that even the internal membrane itself had been destroyed by ulceration.

The vein on which the operation had been performed, was of course examined with peculiar care, to ascertain whether inflammation of it had been excited. The only unusual appearance observed, however, was a darkish red discoloration of the inner membrane, for about half an inch above, and a line or two below the wound. This was seated beneath the surface of the vessel, and, at first glance, looked like the stain of a coagulum which had formed in this part of it. There was no thickening of the coats of the vein, no effusion of adhesive matter, no appearance whatever of a widely spreading inflammation; above and below the spot, the vessel appeared perfectly healthy, as it still does in the preparation of it now before me. If there had been any genuine inflammation at all, it certainly had been slight, and was confined to the vicinity of the wound.

Remarks.

There are various reflections which suggest themselves on considering this case, some of which I may be permitted to notice.

1. It will be observed, in the first place, that this poor fellow fell a victim to exhaustion, notwithstanding the supply of blood which he had received, about fifty-six hours before. When we are considering this fact, however, it must not be forgotten, that the quantity of the injection was very small in comparison with the high degree of inanition. It is wonderful what large quantities of blood may be lost, without immediate danger to life, provided the blood-vessels have time to accommodate

themselves to the evacuation. Repeated venesections afford us a familiar instance of this, as well as bleedings from the womb.

I am indebted to Mr. Lewis Hensley, formerly a student at the united hospitals, for an authentic and extremely intelligent account of two cases of copious blood-letting, which it may be proper to notice here. The patients were two robust countrymen, of the middle size, and laboured under thoracic inflammation. From each of these men, Mr. Hensley, himself, drew off by venesection more than a gallon and a half of blood, he weighed it carefully, in the course of five days; and during the whole of this time they took little aliment besides barley-water; yet both eventually recovered, without any alarming symptoms of inanition. Brazier was a man but little below the middle size. In his case the waste of the blood had been very gradual; and at the time when we operated, it had been carried to the highest pitch compatible with the remains of life. Under these circumstances, I believe a gallon and a half to be the lowest estimate of the deficiency. Indeed, when the extreme emaciation of the patient, and the contraction of the vascular system are considered, together with the gradual manner in which the blood had been wasted away, we shall not, perhaps, appear guilty of exaggeration in rating it much higher; possibly it more nearly amounted to two gallons than one and a half. But even if we take the lowest estimate, twelve or fourteen ounces will appear a very inadequate supply; nor is it to be wondered at, that, after a great part even of this small pittance had been consumed for nutrition, in the course of the next twenty-four hours, the patient should relapse into that state of inanition from which the operation had so imperfectly liberated him.

In alluding to the causes of this exhaustion, I have said little, it will be observed, of the excitement which occurred the day after the operation; because, although this was so inconsiderable as to be in a manner dubious, its effects in contributing to wear out the little remains of the patient's strength, are too obvious to require a comment.

2. The foregoing reflections naturally lead us to inquire, whether the life of our patient would not have been further

prolonged, if a larger quantity of blood had been infused at first, or if the injection had been early repeated; for instance, on the second day? Upon this point there may now be room for a difference of opinion; but it must, I conceive, be admitted, that with the information which we possessed at the time of the injection, the method of operating adopted was upon the whole the most prudent.

It is hardly necessary to remark, that it would have been very unwise to have thrown a large quantity of blood, for example two or three pints of it, into the veins at once. The patient was exhausted; the heart and vessels were feeble; their capacity was become contracted; we had no former experience of the effects of the operation; unexpected and fatal symptoms might, perhaps, have been occasioned by it.

Nor can we deny, that it would have been scarcely less imprudent to have repeated the operation upon the second day. In the memoir already alluded to, I have taken occasion to shew that dogs, resuscitated by transfusion, may, under certain circumstances, die a day or two afterwards.* With this fact before us, it seemed a necessary precaution that we should defer the second injection till the third day at least. It was thus only that we could ascertain whether any symptoms prohibiting a second trial of the operation would ultimately arise from the first.

But the history of this operation has a further claim on our attention, as it elucidates some important points connected with the injection of blood.

1. In the first place it shows that the operation is very easy. A little tube and a syringe were the only novel instruments required; and although this injection was a first attempt, not a single difficulty occurred.

2. It further proves, at least with all the force of a solitary fact, that the infusion of human blood by the syringe is unattended with danger, provided the blood be not suffered to lie at *least* above a minute in the cup. No unfavourable change was produced by the operation at the time, nor was there I

* When the human blood, or that of the sheep has been substituted in large quantities for their own.

think a single morbid symptom observed during the next fifty or sixty hours which could be fairly attributed to it.

3. To this I may add, that the case gives additional strength to the opinion that human blood, although transmitted through the syringe, may still retain a positive fitness for the animal purposes. In this instance it will be observed, that the strength was recruited by it; that the pulse became larger and the temperature of the body warmer. The man himself felt that he was revived; and I think the spontaneous evacuations from the bowels, and the returns of appetite, especially as they seem to have arisen from an improvement of the alimentary secretions, are further proofs of the little injury which the blood had sustained from passing the syringe. Whether blood injected in this manner, so as to supply the vessels directly, remains so far unimpaired in its qualities, that it will supersede the necessity of a supply by sanguification, the case did not enable us to ascertain, but the doctrine is plausible; there is nothing in it obviously at variance with sound reasoning, and the general tenor of the facts related certainly gives some little countenance to it. Observation, however, and experiment, the sole basis of a solid physiology, can alone solve this problem; nor can any labour, I conceive, be ill laid out which is employed in investigating a point of such importance. And are there not at this moment many patients in our hospitals sinking under inanition, who, if the experiment were explained to them, would be grateful that it should be tried? And would not this experimental remedy secure to some of them at least the only remaining chance for life? And is there a principle in the physiology of nutrition which it is of more importance to establish? Who can tell the various diseases in the management of which it may perhaps be applied? Half the labour laid out by Spallanzani on a single dissertation, would probably establish the affirmative; and I had almost added, that the naturalist who fairly succeeds in proving it, whether by observations on the human subject, or experiments on the brute, will be found, perhaps, hereafter, when this discovery has been matured, and applied to all the medical purposes to which it is adapted, to have conferred no inconsiderable benefit on mankind. This

consideration offers a noble incentive to exertion, and cannot want its due influence over an elevated and truly benevolent mind.

Case of Bronchocele, in which the Superior Thyroideal Artery was successfully tied. By HENRY COATES, ESQ. Member of the Royal College of Surgeons, and Surgeon of the Salisbury Infirmary. Communicated by MR. ASTLEY COOPER.

[From the Medico-Chirurgical Transactions. Vol X.]

AMONG the greatest improvements in modern surgery, none have exceeded, or perhaps equalled, that of the surgery of arteries. To the enterprising genius of modern surgeons are we indebted for the information which enables us to perform operations on them successfully, in cases which were formerly considered impossible, and by which many valuable lives have and will be saved.

The following case is recorded, not for any extraordinary difficulty in the operation, but in order to show that in some cases the thyroideal arteries may be tied with advantage; not that such cases are common, nor would I state any thing to encourage the juniors of our profession in attempting this operation, unless the vessels, as in this case, be sufficiently apparent, and readily within reach.

My friend and colleague, Dr. Grove, having mentioned to me a case of an out-patient of his, of the Salisbury Infirmary, with bronchocele, in which the tumor was pressing on the trachea, and very considerably impeding respiration, and the superior thyroideal arteries being large, and pulsating, suggested the possibility of their being tied with advantage. Elizabeth Spratt, æt. 17, was in consequence admitted an in-patient. The thyroid glands were large, and pressed on the trachea, so as to render respiration and deglutition extremely difficult: and the noise she made was so great that it might be heard at a very considerable distance. The superior thyroideal arteries were large, and pulsated strongly. Her general health was extremely good.

It was agreed, on consultation, that a ligature on the artery afforded the best chance of advantage to the patient. Accordingly, on the 29th of December, I cut down on the left superior thyroideal artery, and having dissected it from its accompanying nerve by means of a bent probe, I passed under the vessel a small round ligature, which was drawn moderately tight and tied. She passed a tolerable night. The next day she complained of head-ache, and there was some swelling of the neck and side of the head, with increased difficulty of swallowing, and some febrile symptoms. These, however, gave way to the abstraction of blood, and antimonial medicines; and in three days she was relieved from all unpleasant symptoms.

The ligature separated on the ninth day, and the wound was completely healed on the fourteenth day. Her breathing was much improved, and the size of the tumour reduced nearly half; and she was so materially benefited, that she was discharged on the 14th February, quite well.

She was to have returned to the infirmary should any of her unpleasant symptoms have recurred; but I presume that the tumor has not again increased, and that she remains so well that she is unwilling to submit to any further operation.



Observations on the Relaxed Rectum. By THOMAS CHEVALIER, Esq. F. R. S & F. L. S. Surgeon Extraordinary to the Prince Regent, and Consulting Surgeon to the Westminster General Infirmary.

[From the Medico-Chirurgical Transactions. Vol. X.]

A RELAXED state of the coats of the intestinal canal admitting of excessive distension, is not of unfrequent occurrence. In most cases of peritoneal inflammation, whether acute or chronic, it is one of the earliest symptoms which arises, and affords a remarkable illustration of the pathological fact, that an inflammatory excess of action in the vessels of a part, is always accompanied with a loss of its tone. In this instance

the inflammation is aggravated by the distension, and that distension rapidly increases in consequence of the adhesions which form between the convolutions of the intestines, and which, by arresting their peristaltic motion, tend greatly to disable them from expelling their contents.

Parts of the canal, however, may become relaxed, and overdistended in consequence of that relaxation, with little or no inflammation, and probably arising from a loss of tone simply. The large intestines seem more subject to this derangement than the small ones, and the transverse arch of the colon is especially liable to it. The extreme distension of the abdomen in tympanites is chiefly produced by the extraordinary degree in which this part is dilated. A less degree frequently accompanies ascites, and sometimes occasions an apprehension in the mind of the physician that there is a much larger quantity of fluid in the abdomen than is actually accumulated. In such instances, the fluctuation is more obscurely perceived, and it is chiefly at the lower part of the abdomen: the enlargement is greater above the navel than below it, and when the upper part of the belly is struck gently by the hand, it gives that peculiar sensation and sound which a membranous cavity filled with air communicates. This circumstance deserves the most careful attention on the part of the surgeon, who might otherwise be induced to tap the patient, and might puncture the intestines by his trocar. I believe, however, where this state of the colon exists, that the ascites is very seldom the principal disease. I have repeatedly objected to operate in such cases, having always found in those which I have had an opportunity of examining after death, that the quantity of fluid has been much less than had been expected, and that it has been irregularly effused among partial adhesions, which have been excited by the irritation of some visceral disease. If it should be thought advisable in such a case to puncture the abdomen, the operation should be performed by the cautious introduction of a lancet through the linea alba below the navel, and not by a trocar.

Dilatations occasionally take place on the sigmoid flexure of the colon, the sacculated subdivisions of which prevent their dilated portions from being equally distended. From this cause scybala are often formed of the excrementitious

matter, and may be retained for a considerable time by the valvular projections of its internal coat, although a tolerably regular evacuation of fæces may go on. These are well known to give rise to dysenteric affections, and inflammations of the inner surface of the bowel; in relieving many of which, copious and repeated glysters are of the most essential service, by softening and washing out the hardened substance.

That the lower part of the rectum is frequently so relaxed as to prolapse externally, is well known; but it does not appear to have been much observed by practitioners that it is also subject, without any external protrusion whatever, to excessive dilatation within the pelvis, and to a semiprolapsus of its upper part into the lower. But this is a state of that bowel which frequently occurs. It is often productive of very distressing symptoms; and it is especially to be remarked, that it is a common cause of that obstinate and habitual costiveness, under which some persons continually labour.

The rectum commences from the colon, close to the last vertebræ of the loins, and passing down into the hollow of the sacrum, it takes the curvature of that bone, in which it lies comparatively loose, invested anteriorly, but not posteriorly, by the peritoneum. When it reaches the os coccygis, it quits the peritoneum entirely, and is connected loosely by cellular membrane to the bladder, to the muscles of the perineum, to the levatores ani, the sphincter ani, and the common integument. This lower portion of the rectum is easily distensible; but while it is in a natural state, the peculiar sensibility of its internal surface speedily excites it, when moderately distended, unless the fæces are unnaturally hard, to expel its contents; and its muscular fibres are competent to enable it to do so, with a very moderate assistance from the action of the abdominal muscles. It is needless to say of how much consequence it is to the general health that the sensibilities of this important organ should remain unimpaired, and that strict attention should be paid to the regular performance of its functions: if this be long neglected, its natural sensibility becomes gradually diminished; it will remain overcharged for an undue time; the energy of its muscular fibres will become impaired, so that a more forcible exertion of the abdominal muscles will be re-

quired to expel the stools; and not unfrequently this will also be insufficient, without some medicine be taken to quicken the action of the whole intestinal canal.

Here perhaps, in a majority of cases, the evil stops. The calls of nature must be obeyed, and therefore persons who feel a difficulty in discharging the excretions regularly, are of necessity excited to employ those means which afford them the requisite assistance.

But in some instances, and these by no means rare, the lower part of the rectum becomes so frequently overloaded, and its irritability in consequence so much diminished, that it becomes excessively dilated, and almost loses the power of contracting upon its contents; the natural consent between that and its upper portion, which is covered by the peritoneum, and remains undilated, is thus weakened and further evil is induced. The superior portion of the rectum and the lower part of the colon, become also overloaded, and the deficiency in the action of these parts calling forth a greater exertion of the abdominal muscles, in the expulsion of the fæces, the upper, undilated portion of the rectum, is forced downwards into the lower and dilated portion, where it may be distinctly felt like a loose bag, of which it is sometimes difficult to detect the aperture; the finger or a bougie being more likely to get entangled at the rim of this elongated fold, than to pass exactly into the continuation of the tube.

It is under these circumstances that the chief evils and perplexities of such cases begin. For the harmony of action between the upper and lower portions of the rectum being destroyed, the stools, instead of being voided with that ease, and in that regular form and mass which is usual in a healthy state of the parts, are expelled with difficulty, in small and irregularly shaped pieces. The repeated efforts made for this purpose sometimes excite tenesmus, swelling of the hæmorrhoidal veins, and an increased secretion of mucus from the inner surface of the intestine; in men, the irritation is often communicated to the prostate gland, and neck of the bladder. In other instances, and especially in females, the parts become so relaxed as to allow of a sufficient accumulation of fæces to fill the whole pelvis; and how unconscious a patient may be of

such an accumulation, the following case, to which others might be added, will sufficiently shew.

A lady who was afflicted with cancer of the left breast, became affected with severe pain in the loins, which confined her to bed. She soon afterwards became unable to pass her urine, which was drawn off at proper intervals by a catheter. The state of her bowels was regularly inquired into, and she always replied that the evacuations were small in quantity, but frequent, and upon the whole, sufficient; and that this had been her ordinary habit for many years. After about a fortnight had elapsed in this way, her attendants noticed a peculiar appearance about the anus, which, on examination, was found dilated to the size of an half crown, by the protrusion of fæces, which had so completely stuffed the rectum, as entirely to choke up the pelvis; and although not hardened, were incapable, from their quantity, of being removed without the assistance of instruments.

Sometimes *hardened* fæces accumulate in a similar manner, and become, from their bulk, incapable of expulsion without artificial aid; and yet that which is softer may pass over this lump in daily evacuations, and thus conceal the real mischief.

In more confirmed cases of the disease, and in persons of a sedentary habit, other bad consequences arise. The upper portion of the rectum being forced down as has been described, by a half intus-susception upon the lower, becomes in time less competent to its own functions, and transmits the fæces irregularly. The lower portion of the colon participates in this difficulty, and is kept in a state of irritation. An obscure heavy pain is felt in the lower part of the loins and the region of the sacrum; and the difficulty and imperfection with which the stools are voided, often gives rise to a suspicion that there is a stricture formed in some part of the intestinal tube; and this suspicion, although entirely groundless, will be confirmed in the apprehension both of the patient and his attendant, if, on making an examination with a bougie, its point should be arrested, as it is very likely to be, in the edge or fold of the semiprolapsed portion of the gut.

Under these circumstances, an increased secretion of mucus from the surface of the colon may take place, to a consi-

derable amount, so as to collect in some of its sacculated portions, and to be voided in a large quantity; and as it then has often a yellowish appearance, it seems as if an abscess had burst, and its contents had been discharged by the anus. But the matter is more tenacious than true pus; it is not mixed with blood; that degree of relief and change from former feelings, which is always felt when an abscess breaks, is not experienced; the discharge does not go on regularly; it is seldom seen above twice or thrice, and then this symptom disappears. I have several times known more than half a pint of this purulent looking mucus so voided, and then there has been no more for several days, or but once, and in some instances it has not recurred at all. A circumstance of this nature is sure to attract the patient's attention, and if mild and demulcent aperients are given, and temporary symptoms properly treated, all goes off: and the patient becoming more vigilant over the state of his bowels than formerly, the parts affected may greatly recover their health and tone, and he may be better than he had been for a considerable time before this occurrence took place.

The state of the rectum I have now been describing, is most common to females, and to persons who, from their habits being generally sedentary, are more apt to overlook the irregularities of its action, and to defer obedience to the calls of nature. Under these circumstances, purgative medicines are mostly resorted to, and the intestinal canal is teased and pained, for the very defective action of that very part of it which is most remote from their influence. The general health also often suffers; all the evils arising from costiveness taking place, and hypochondriacal dejection and gloom oppressing the mind.

In cases of this kind, the principal and most certain relief is to be obtained from the proper employment of glysters, the composition of which is to be regulated by circumstances. At first those of a mild aperient nature should be preferred, and thrown up by a syringe; and these should be repeated *after regular intervals*, so as to re-accustom the rectum to empty itself in an *habitual* way. Gruel, the decoction of mallows, broth, or milk with some honey, will answer the purpose very well; but

more permanent good will be derived by using afterwards the infusion of chamomile, or the old decoction *pro fotu*, which, by gently stimulating the torpid surface of the bowel, may bring on a proper contraction of its coats. Where the very lowest part of the rectum continues so dilated as to allow the upper still to descend, from four to six ounces of a strong decoction of oak bark, or an infusion of galls, thrown up as gently as possible every night, will be attended with the most beneficial effects. If this be not readily retained, a little starch, or a few drops of tincture of opium, or both, may be added to it; and care should be taken at the same time so to regulate the diet, and medical treatment, that the upper part of the intestinal tube may be excited to due action, regularly, but not violently.

Should inflammation take place, which sometimes happens, at the prolapsed part, so as to consolidate the surfaces together, a permanent stricture or obstruction is formed, which, by the frequent irritation to which it must be unavoidable exposed, may take on a cancerous character, and be productive of the most disastrous effects.

A Memoir on some new Combinations of Prussic Acid. By his excellency the Count LE MAISTRE, of St. Petersburg.

[From the Annals of Philosophy, for December, 1819.]

FERRUGINOUS prussic acid, when assisted by heat, combines with different substances, which, while cold, do not exhibit any affinity for it; and all these combinations are blue, like prussiate of iron. Copper itself, whose dark carmine red prussiate is well known, is also capable of forming a blue prussiate according to the state of oxygenation in which it is, and by means of processes which will be explained in the course of this memoir. The following are these different combinations in the order in which they presented themselves to me during my experiments.

I. Prussiate of Starch.

While engaged in making a small quantity of the iodide of starch in a porcelain cup, I employed in the preparation water which contained prussiate of potash and muriatic acid in solution. The mixture which I exposed to the heat of a lamp remained colourless till the heat became sufficiently strong to dissolve the starch. It then suddenly assumed a very perceptible green colour. As I was aware of the substances contained in the mixture, I immediately repeated the experiment in such a way as to remove all suspicion of the presence of iron. I took 10 gr. of fine starch which I mixed with an ounce of the water of the Neva. I added 20 gr. of prussiate of potash, and boiled the whole in a matrass. The liquid became transparent, and remained colourless during the whole time of boiling. I then added muriatic acid, which made it immediately assume a green colour as if I had poured into it a solution of iron. The precipitate in 48 hours became of a fine deep blue. I repeated the experiment with different starchy substances, as arrow root, sago, potato starch, and always obtained the same result. Thus starchy bodies, when dissolved in water at the temperature of 212° , have the property of forming blue prussiates like iron.

II. Prussiate of Gum Arabic.

I made the same trials with some analogous substances. Ten grains of gum arabic and 20 gr. of prussiate of potash were dissolved and heated together in a matrass with an ounce of water. The mixture remained transparent and colourless during the boiling. Boiling muriatic acid made it pass to a deep green. The precipitate was abundant, and fell down rapidly.

The prussiate of gum is less black than pure prussiate of iron. It is insoluble in water, and in muriatic acid. Concentrated sulphuric acid dissolves it, and destroys the colour; but it appears again when the solution is diluted with water. The prussiate in that case precipitates with all its lustre, and when it is washed and dried, we cannot from its colour distinguish it from common prussian blue.

III. *Prussiate of Sugar.*

The same process and the same quantities being employed with sugar yielded likewise a blue prussiate, which was at first green, and which, when exposed to the air, assumed a lighter shade than the preceding blues.

To avoid useless repetitions, I shall merely say, that having experimented in the same way with solutions of gelatin, of cochineal, of tannin, of soap, and even of white wax melted and mixed with the prussiate of potash, on decomposing the prussiate by muriatic acid while boiling-hot, I always obtained blue prussiates; while none of these compounds could be formed at the temperature of the atmosphere.*

Of all these substances cochineal is the one which resists most the action of prussic acid, the combination not taking place till after half an hour's boiling. This colouring matter, which the mineral acids and alkalies do not decompose, becomes green by its union with prussic acid. The precipitate resembles the juice of herbs, and soon passes into a perfect blue. The residual liquid, when there is an excess of cochineal, preserves its red colour; the prussic acid taking only the quantity necessary for its saturation.

IV. *Prussiate of Charcoal.*

As all the substances of which I have spoken contain a great quantity of charcoal, it was natural to try the same thing with this last substance. To obtain a charcoal in a state of minute division, I charred white paper free from size by putting it into concentrated sulphuric acid, and exposing the acid to heat. The black matter thus formed was diluted with a great deal of water, and decanted without filtration into a matrass, which I placed over the flame of a spirit of wine lamp. I then poured into it while boiling a portion of prussiate of potash, and the mixture became dark green. The prussiate did not assume a blue colour till after eight days exposure to the air.

* It may be thought that these substances are coloured by the iron of the prussic acid as by a tincture; but the gum and starch become insoluble in water, which indicates a true combination, and the cochineal becomes green, and not violet.

That the paper may be completely charred by the sulphuric acid, it must be slightly moistened before being plunged into it.

This, however, was not a pure prussiate of charcoal; for as the sulphuric acid chars the paper without effervescence and without smell, this species of solution contains all the principles of the ligneous substance.

To obtain a pure prussiate of charcoal, I took 30 gr. of the charcoal of the birch which had remained for a fortnight in an alkaline ley. I pounded it while still moist upon an unpolished piece of glass, with concentrated sulphuric acid. I put altogether three ounces of water, and boiled the mixture in a matrass. I then added to the liquid 60 gr. of prussiate of potash, continued the boiling for about an hour, adding water in proportion as it evaporated. A dark-green prussiate was formed, which became blue after five or six days exposure to the air.

If the dried prussiate is not of as fine a blue as the best prussian blue, it must be repeatedly moistened and dried, which will produce the colour desired.

When the process was repeated with muriatic acid instead of sulphuric, the result was the same; but when newly formed charcoal was employed, no combination took place.

The formation of this prussiate was accompanied with frequent anomalies, depending doubtless on the different states in which the charcoal is at the time in which it is employed, and upon minute circumstances which I probably overlooked. If instead of boiling the mixture for an hour, as I have described, it be poured after two minutes boiling into a kettle furnished with a ground-stopper, and placed well corked in a stove heated to 122° or 144° , it will be found completely formed after an interval of eight or ten hours. I put my bottles in the evening into a Russian stove, and next morning found a green prussiate formed.

It cannot be doubted that if the charcoal could be presented to the prussic acid in a state of perfect solution, the combination would take place, instantly, as happens with the gums.

When the prussiate of charcoal is dried, it has a fine brilliant blue colour, like prussian blue containing alumina; which appears singular, if we consider the black colour of charcoal. It dissolves in cold sulphuric acid of commerce without losing

its colour. The solution appears green when the acid begins to act, as is the case with indigo; and when it is diluted with water, the prussiate precipitates.

If we employ very concentrated sulphuric acid, the colour disappears on solution, and the liquid becomes yellowish; but when water is added, the prussiate precipitates of its original colour. It is insoluble in water and in muriatic acid, which merely divides it.

These properties of prussiate of charcoal belong likewise to those of starch, gum arabic, and cochineal, which are probably of the same nature.

The analogy which exists between this prussiate and indigo is remarkable. They have both the same colour. The absorption of oxygen makes them both pass from green to blue. They are both soluble in sulphuric acid, and finally they are composed of the same principles; namely, of a great proportion of carbon combined with azote, hydrogen, oxygen, and iron; but in proportions, or, perhaps, only in an order of composition which gives them different properties.

After having formed blue prussiates with the oxide of carbon and several of its compounds, I thought of subjecting to the same process the earths which had not yet been united to this acid, and the simple substances which I could procure.

V. Prussiate of Sulphur.

As sulphur unites easily with potash, I formed a sulphuret with 10 gr. of flowers of sulphur and 20 gr. of prussiate of potash in a small matrass, which I heated by a spirit of wine lamp. When I thought that the sulphuret was formed, I broke the matrass in a vessel of boiling water mixed with muriatic acid. The sulphuret, which had acquired no colour, was decomposed in the water, and the prussiate of sulphur made its appearance in fine green flocks in the acid liquid. A few days are sufficient to give it a fine deep blue colour.

This prussiate has a green colour only when the sulphuret is made in close vessels. If it be made in an open crucible, and if it be allowed to burn for some time stirring it with a glass rod, the prussiate is blue the instant it is developed in the muriatic acid; so that the portion of the sulphur which burns is

sufficient to bring the rest to the point necessary for forming prussian blue. I shall prove afterwards that no prussiate becomes blue unless oxygen and iron enter as constituent parts of prussic acid.

The following process likewise furnishes a prussiate of sulphur of a very beautiful colour.

I boiled in water one part of hydrosulphuret of potash and one part of prussiate of potash. I added weak muriatic acid at a boiling temperature, which immediately produced the green combination. No sensible portion of sulphuretted hydrogen was developed.

VI. *Prussiate of Phosphorus.*

Phosphorus has a great deal of affinity for prussic acid. To produce a combination it is sufficient to boil some grains in a solution of prussiate of potash, and to pour muriatic acid into the liquid. It becomes muddy, and assumes a green colour. The boiling is to be continued till no more melted phosphorus can be seen at the bottom of the matrass. The prussiate has then the colour of Scheele's green, and in process of time becomes blue.

It would seem that phosphorus can unite in several proportions with prussic acid. If we employ a weak solution of prussiate of potash, the colour is less intense, and passes with difficulty to blue. If we employ a concentrated solution, and pure muriatic acid, the prussiate, when dried, is absolutely black, and becomes luminous. The excess of phosphorus burns slowly, and leaves a dark-violet coloured matter, which gradually changes into a perfect blue.

Prussiate of phosphorus, when well made, is more beautiful than prussiate of iron, and is neither purple nor greenish.

VII. *Prussiate of Gold.*

A solution of ducat gold diluted with much water, treated during its boiling with prussiate of potash, gave immediately a copious blue precipitate, which was deposited rapidly, and had a very fine colour.

VIII. *Prussiate of Silver.*

As all the nitric acid solutions have a tendency to give a

green shade to the prussiates derived from them, we obtain only an imperfect blue with nitrate of silver; but we get a very fine blue by operating on the muriate of silver newly precipitated by muriatic acid. It is washed upon the filter, and boiled in water, before it is coloured by light. Muriatic acid is added, and then prussiate of potash. A prussiate is formed of a clear and brilliant blue, which becomes gradually more intense. This is the prussiate of muriate of silver.

IX. *Prussiate of Tin.*

The disoxygenizing property of muriate of tin renders the formation of blue prussiates with it difficult; but we obtain them with facility when we operate upon the oxide of tin precipitated from the nitrate by an alkali, well washed, and employed while still moist. The process is the same as in the preceding experiment, excepting that the prussiate of potash must be added to the mixture before introducing the muriatic acid.

X. *Blue Prussiate of Copper.*

Copper presents the singular phenomenon of being able to furnish two permanent prussiates of different colours.

If we pour a strong solution of prussiate of potash on pure copper filings, and then add a sufficient quantity of muriatic acid to decompose the prussiate, and to dissolve a portion of the copper, the metal dissolved in presence of the prussic acid forms a very beautiful blue prussiate. This combination does not, like the preceding, depend upon the temperature. Heat merely increases the rapidity of its formation.

When we treat granulated tin in the same manner, we obtain likewise a blue prussiate, but we must employ nitromuriatic acid and heat.

XI. *Blue Prussiate of Mercury.*

To form the blue prussiate of mercury, the red oxide of that metal is boiled in water, and a solution of prussiate of potash previously decomposed by muriatic acid is added to it. The iron contained in the prussic acid changes its condition so as to become proper for forming the blue insoluble prussiate

of mercury; while the acid without iron forms a colourless soluble prussiate. This proves sufficiently that iron is a constituent part of the prussic acid employed in these experiments. The blue prussiate of mercury, when first formed, is light green, and passes slowly to blue by exposure to the air.

XII. *Prussiate of Iodine.*

If we boil iodine in a solution of prussiate of potash, adding muriatic acid, we obtain a blue prussiate with a shade of purple of the greatest beauty. When first formed, it is light green, and requires a long exposure to the air to give it a blue colour.

XIII. *Prussiate of Alumina.*

The opinion that alumina forms no combination with prussic acid is so generally established, that when I began my trials with this earth, I had little expectation of success. In fact the sulphate of alumina precipitated by prussiate of potash gives no colour either when cold or hot. However, I observed that when the salts in the mixture were concentrated, the alumina, after long boiling, assumed a very distinct blue colour. The following trial furnished me with a more complete combination.

I pulverized together equal parts of sulphate of alumina and prussiate of potash. I introduced them into a small matrass without water, and heated them over a spirit of wine lamp to cause them to melt in their water of crystallization. The mixture became blue as soon as it began to swell. I regulated the heat in order not to decompose the prussiate of alumina, and when the tint appeared to me equal through the whole mass, I diluted it with boiling water. The prussiate, which was of a light and dirty blue, immediately assumes a fine colour, and forms a copious precipitation similar to prussiate of iron; but which seems somewhat soluble in water, to which it gives a greenish tint.

As alumina has been long employed by the makers of prussian blue along with prussiate of potash without any such combination being observed, I repeated the same experiment several times to be certain of its accuracy, and I likewise made another experiment which appeared to me conclusive.

I precipitated the alumina from the sulphate by means of

carbonate of potash; I washed the earth on a filter, and dissolved it, while yet in a gelatinous state, in muriatic acid. I heated the solution, which was very acid, till it boiled, and then threw into it hot boiling prussiate of potash. The effect was the same as if it had been poured into a solution of protosulphate of iron. Instantly a very fine green colour was developed, which in two days was changed into an intense and perfect blue. We have seen that sulphate of alumina exhibits different phenomena, which is doubtless the reason why this compound has never been observed in the prussian blue manufactories.

Alumina then has the property of forming a blue combination with prussic acid, but only at a temperature not lower than 212° .

XIV. *Prussiate of Silica.*

To form this prussiate, I pounded in a glass mortar 50 gr. of white glass from a barometer tube with 150 gr. of calcined potash. I put the mixture into a covered crucible, and kept it red hot for an hour. A frit was formed, which was almost completely soluble in muriatic acid. I filtered the transparent and colourless solution, and raised it to a boiling temperature. I then added 40 gr. of prussiate of potash, which gave a deep-green colour to the liquid. After eight days exposure to the air, it became a blue prussiate.

The same process performed with pulverized quartz, prepared for porcelain, gave the same result. The prussiate of silica is blacker and less beautiful than prussiate of alumina.

XV. *Prussiate of Carbonate of Lime.*

Some salts, insoluble, or scarcely soluble in water, form likewise blue combinations with prussic acid without undergoing decomposition.

I boiled water containing carbonate of lime diffused through it, and mixed with it a liquid containing prussiate of potash with an excess of muriatic acid. The prussic acid immediately united with the carbonate, and the prussiate formed became bluish-black after some days exposure to the air.

It was insoluble, in cold muriatic acid. Concentrated sul-

phuric acid disengaged a great deal of carbonic acid, and formed a sulphate without destroying the blue colour.

XVI. *Prussiate of Sulphate of Lime.*

Sulphate of lime appears to me one of the substances which has the greatest affinity for prussic acid of those which I tried. I precipitated muriate of lime by sulphuric acid diluted with seven or eight times its weight of water. I heated it to the boiling temperature, and added prussiate of potash. There was immediately formed a prussiate of a fine green, which passed very speedily to blue.

Calcined sulphate, or fine plaster of Paris, may be employed to form this prussiate. It is diffused through a great quantity of water, and then a solution of prussiate of potash having an excess of muriatic acid previously added to it, is poured in. Boiling causes it to assume a green colour.

When the prussiate of sulphate of lime has a strong blue colour, and contains a sufficient quantity of prussic acid, it is insoluble in water, and in the mineral acids: 15 gr. of prussiate of potash were sufficient to give to 100 gr. of calcined plaster a very distinct blue colour.

The sulphates of strontian and barytes newly precipitated and treated like that of lime, likewise furnished deep blue prussiates.

XVII. *Prussiate made with common White Clay.*

The dry earths and the metallic oxides in powder, when they are sufficiently divided, combine very well with prussic acid, when assisted by heat.

I mixed with water, fine white clay employed at St. Petersburg for the manufacture of porcelain, and which does not contain an atom of iron. I decanted off the finest parts, which I boiled with prussiate of potash, without producing any change in the colour. The addition of muriatic acid made the mixture become green. After a quarter of an hour's boiling, I poured the prussiate formed into a plate. It had a fine green colour, and became blue after some days exposure to the air.

This prussiate may become useful in the arts. It has the

advantage of being always of a distinct blue colour, whatever be the proportion of acid employed to form it. This is not the case with iron, which gives yellow and green subprussiates.

XVIII. *Prussiate formed with Greenish Gray Clay.*

This clay, which is found abundantly in the neighbourhood of St. Petersburg, and which I conceive to be coloured with a little chlorite, when treated like the white clay, gave also a beautiful blue prussiate. As it contains iron, its prussiate is nearly the same as common prussian blue. It requires longer boiling than the preceding, because it is difficult to destroy the green colour of the earth; but when it is formed, it is perfectly similar to the finest prussian blue of commerce.

Observations.

When we review the facts contained in this memoir, we may observe, that when the combinations of prussic acid with carbonate of lime, sulphates of lime, barytes, and strontian, pass from green to blue by the slow absorption of oxygen from the atmosphere, it is not, in all probability, the bases of these prussiates which are oxidized; for the carbonate and the sulphate of the earths are not capable of uniting with an additional dose of oxygen. It is probable then that it is the prussic acid itself which unites with the oxygen in this case.

We find a proof of the oxidation of this acid in its combination with indigo. The boiling sulphate of indigo precipitated by prussiate of potash is all at once disoxygenized, and assumes a green colour, as when it is treated by sulphate of iron in the dyer's vat.

It is likewise very probable, that in all the blue metallic prussiates the prussic acid is oxidated, while the bases are at a minimum of oxidation, and this is evident for the blue prussiate of copper. We see in fact that the protoxide and peroxide of copper in all their solutions form red prussiates; while in the blue prussiate (experiment 10), the copper dissolved in contact with the prussic acid is seized upon in a nascent state, at a degree of oxidation which is doubtless less than

that of all the known oxides of copper, which give red prussiates.

If the peroxide of iron gives a blue prussiate at the instant of its formation, the reason is that it contains enough of oxygen to saturate the acid while it passes into the state of protoxide, while the peroxide of mercury which contains only 0.10 or 0.15 of oxygen forms a green prussiate. Now as it is known that mercury is incapable of combining with a greater quantity of oxygen, it follows that when the green prussiate passes into blue, it is not the base which absorbs oxygen, but the prussic acid itself.

Sulphur, phosphorus, and carbon, form green prussiates, because they cannot furnish the requisite quantity of oxygen to the prussic acid. It is obvious that these radicals cannot pass to a maximum of oxidation, when the prussiates change from green to blue, because, when saturated with oxygen, they become acids. This is the case also with those substances which contain much oxygen in such a degree of union that the prussic acid is unable to abstract it. These substances combine green, as the earthy sulphates, and the oxides of aluminium and silicon.

By means of this theory, which explains all the facts, the phenomena of the two prussiates of mercury, one soluble and colourless, the other blue and insoluble, are easily understood. When in order to make the soluble prussiate of mercury, we boil prussian blue with red precipitate, a change of bases takes place by double affinity. The mercury has a very great affinity for prussic acid; and the iron of prussian blue, which is at a minimum of oxidation, has likewise a great affinity for oxygen. Hence it happens that the iron deprives the acid of oxygen, and passes into the peroxide; while the mercury seizes the pure prussic acid without oxygen and without iron. In this last state the prussic acid drawn from mercury has no *cyanic* power, and remains mixed with sulphate of iron without forming a prussiate.

I shall terminate this memoir by an experiment, the result of which gives great probability in this conjecture.

If the prussic acid, when it combines with the peroxides,

has really the property of absorbing a portion of their oxygen, and of bringing them to the state of protoxide, it ought to act in the same way on the oxide of the red prussiate of copper, and make it pass to blue, on the supposition that in the blue prussiate the oxide of copper is at a minimum of oxidation.

To determine this point, I mixed beautiful red prussiate of copper, newly precipitated and washed on the filter, with water. I added a little muriatic acid, and boiled the liquid. The colour appeared to become more red. I then threw in some crystals of prussiate of potash, and had the pleasure of seeing the mixture speedily pass to the finest blue.

This experiment seems to me to furnish a double proof, both that prussic acid does not produce the blue, except when it is oxidized, and that the bases of the metallic prussiates are at the minimum of oxidation. We may, therefore, distinguish three different well marked states in prussic acid. 1. The pure acid without oxygen, and without iron, such as it exists in the soluble prussiate of mercury. 2. The ferruginous acid without oxygen, such as it exists in the prussiate of potash, and in the green prussiates. 3. The ferruginous and oxygenized acid, such as it exists in all the blue prussiates.

Spontaneous Ptyalism, accompanied by a diminished Secretion of Urine. By Dr. Prout.

[From the Annals of Philosophy, for December, 1819.]

THE subject of this singular case was a woman upwards of 60 years of age. The ptyalism was constant and excessive, amounting to several pints in the course of the day, while the urine was reduced to a few ounces. Its source appeared to be the whole of the apparatus destined to secrete saliva. The woman had lost her appetite, and her strength appeared reduced, but in other respects nothing remarkable was observed.

The taste of the fluid was described as *urinous*. It was opalescent, very slightly ropy, and foamed when agitated. Its

specific gravity was 1005·5. It restored the blue colour to reddened litmus paper, and faintly reddened turmeric paper. Hence it contained a free alkali, to which was doubtless owing the *urinous* taste above-mentioned.

When exposed to heat, it coagulated. The soluble salts of lead, mercury, and silver, when added to it, caused precipitates. The addition of the mineral acids also caused precipitates. Even dilute acetic acid produced a copious precipitate, but when prussiate of potash was afterwards added, no precipitate took place. Hence the animal matter, though evidently retained in solution by the free alkali present, was not albumen; but appeared to be the peculiar matter secreted by the salivary glands, perhaps a little altered in its nature.

One thousand grs. evaporated to dryness at a temperature between 212° and 300°, left 8·65 grs. which were found to consist of

Animal matter above-mentioned	- - - -	3·33
Animal matters soluble in alcohol and similar apparently to what are usually found in the blood	- - - - - - - - - -	1·06
Sulphuric acid	- - - - - - - - - -	0·90
Muriatic acid	- - - - - - - - - -	0·75
Phosphoric acid	- - - - - - - - - -	0·06
Alkaline matter, consisting partly of soda and partly of potash	- - - - - - - - - -	2·55
		<hr/> 8·65

With respect to the relative proportions of the potash to the soda, or how they were distributed among the acids, I did not attempt to ascertain. But it is evident that the acids were not sufficient to neutralize the alkalies present, which accounts for its sensible properties, as detailed above. When the animal matters were burned, they left a minute quantity of the earthy phosphates.

The urine of this woman was of an amber colour, slightly opaque. Its specific gravity was 1013·1. It contained crystals of uric acid, and reddened litmus paper more strongly than usual. It contained much less urea than natural, but a

large proportion of a brown animal substance, which appeared to render it very prone to decomposition, especially when exposed to heat.

This case occurred to my friend Dr. Elliotson, at St. Thomas's hospital. With the view of increasing the flow of urine, diuretics were given. These produced the desired effect. The urine was rendered more natural and copious; while the salivary discharge was proportionably diminished.

It is not unlikely that cases of this nature have been mistaken for *discharges of urine from the mouth*. In the present case, it appears that the woman herself considered that the fluid came from the stomach, and was of an urinous nature. She had laboured under similar attacks before, but they were less severe, and after some time had ceased spontaneously, and her appetite and strength had returned as usual.

The Application of the Cupping-glass recommended to expedite the Discharge of Matter from Abscesses of the scrofulous kind. By FRANCIS BUSH, Esq. Member of the Royal College of Surgeons.

[From the London Medical and Physical Journal.]

ABSCESSES of the subacute or scrofulous kind are generally indolent and tedious in their progress, but little under the control of constitutional means or the usual topical applications; and it often happens that the patient suffers a very protracted illness before the discharge of the matter takes place; and, where it is deep-seated, it frequently runs along the course of the contiguous muscles, producing extensive lesion and constitutional mischief. In the management of this disease I have been induced to adopt a plan to solicit the early discharge of the matter, which has been attended with a success beyond my most sanguine expectations. As I have nothing new to offer on inflammation, I shall not remark on the early treatment of abscess; but shall take up the subject where it is quite clear that matter is formed, and requires to be discharged.

One very striking advantage to be derived from the application of the measure I suggest, is, that you effect the discharge at the most depending part of the abscess. I shall subjoin two cases to illustrate my plan, which I am confident is capable of doing much service, if judiciously applied.

CASE I.—Mr. D. aged 24 years, was, after violent exercise, in the heat of July 1818, attacked with shivering, succeeded by deep-seated pain in the lumbar region. Leeches, general bleeding, and the usual methods of subduing local inflammation, were tried, without success. Matter formed; and it was invited to the surface by poultices of various kinds for some weeks, to but little purpose. In this state of the case, I thought, by taking off the atmospheric pressure from the most depending part of the tumour, I might produce some excitement in the parts, and effect an early discharge: for that purpose I applied a very small cupping-glass, and allowed it to remain on for an hour; it produced a little pain only. It was repeated in the evening; and for four successive days it was continued in a similar way. The removal of the glass was succeeded by a warm poultice. By these means, in five days, I effected a discharge from the most convenient part of the abscess, which might not have been produced under the ordinary means of treatment, in as many weeks.

CASE II.—A young friend of mine was attacked with inflammation in the groin, which went on to suppuration: it was not attended with pain, and after several weeks, there was no prospect of an opening being effected by the powers of nature. He was averse to its being opened by the lancet; and, indeed, the indurated state of the parts rendered it improper in this stage of the case: a small cupping-glass was therefore applied daily for three or four days, and the parts covered with a poultice after its removal. The discharge of the matter was thus effected; and the parts healed in a very short time.

I considered both these cases as scrofulous. In phlegmonous abscesses the rapid progress of the disease renders this plan unnecessary, and the high irritability of the parts would make its application improper.

Frome, October 1st, 1819.

SELECTED REVIEWS.

Elements of Medical Logic, illustrated by practical Proofs and Examples; including a statement of the Evidence respecting the Contagious nature of the Yellow-Fever. By SIR GILBERT BLANE, Bart. Fellow of the Royal Societies of London, Edinburgh, and Gottingen; Member of the Imperial Academy of Sciences of St. Petersburg; and Physician to the Prince Regent. 8vo. pp. 219. London. 1819.

[From the London Medical and Physical Journal, for October, 1819.]

THAT a system of logical principles of a science so interesting to human happiness as that of medicine should not have hitherto been formed, by which the study of it might be reduced to methodic order, rather than, as is the case, pursued in a more or less irregular and indeterminate way according to the disposition of each individual, is a circumstance that may appear extraordinary to persons who have not contemplated this subject in a sufficiently accurate and comprehensive manner. In order to place it in its proper point of view, we should first determine what it is that constitutes the logic of a science, and how the logical principles of other sciences have been formed; for, it should be understood, physics, as well as metaphysics, may each have their system of logic.

Poetry and music present themselves as well adapted for the illustration of this enquiry. Above twenty ages have elapsed since their logical principles were instituted, and the same principles have maintained their original authority down to the present period, amidst all the revolutions of human sentiments, under the different social and political customs and habits that have intervened.

The man of pre-eminent genius, who has collected from any objects the most accurate and comprehensive series of ideas,

the mutual relations of which he has also determined, has acquired a system of knowledge of such objects that must be considered relatively perfect. To trace those ideas and the order in which he has acquired and developed them, must, then, display all that is known respecting the subjects of them, and, at the same time show the method by which such knowledge may be most accurately and readily obtained: it is this, we think, which constitutes the logic of a science.

From this view of logic, it must be evident that knowledge must be obtained, before the principles best adapted to direct men in general in its acquisition can be instituted. Such has been the fact with regard to poetry and music.

HOMER had acquired the most perfect series of ideas of the good and beautiful, the καλοκαγαθου, of poetical sentiments, before a system of *Poetics* was formed: ARISTOTLE only described the course taken by Homer. EURIPIDES had impressed the mind of man with the most vivid feelings of awe and astonishment, before LONGINUS could have written his treatise on the *Sublime*.

It was the same with respect to *Music*. The ear of PYTHAGORAS had enabled his mind to discern the relations existing between certain sounds, before he applied the relations of numbers to them to explain the nature of harmony.

If this notion be correct, and further illustrations of it might be readily adduced, but little need be said to shew why logical principles have not hitherto been instituted respecting medical science.

Our ideas of the physical, or rather mechanical, construction of the human body, may perhaps be sufficiently accurate and comprehensive for such a purpose; but the physiology of it is yet obviously very far from being in so perfect a state; and its pathology is still less advanced, although this was cultivated in a correct manner at a very remote era. But subsequent enquiries deviated from the course taken by Hippocrates; vain researches after proximate causes were made, instead of endeavours to trace the history of phenomena; and though at length BACON fully displayed the folly of this conduct, it is only lately that his precepts have been followed by the generality of physicians. Of therapeutics our knowledge is far from being

accurate, comprehensive, and well-determined; and it is, on many important subjects, vague in the extreme. We have discerned a connexion between certain remote results and certain agents, but we have in but a very few instances traced the progressive phenomena on which they depend. We do not mean to say, that it is to be regretted that it has not been ascertained in what manner medicinal substances primitively affect the part of the animal body to which they are applied; but that we have not discerned the order of the secondary and subsequently progressive trains of results, without which our knowledge of this subject is too imperfect to serve as a basis for any logical principles. Thus, as an example, we know not in many instances whether medicinal substances applied to the stomach affect remote parts of the system by being carried to them through the medium of the fluids, or solely by organic, or functional, connexion. Of one probably highly important subject, what may be termed *atmospheric* influence on the body, but very little is known.

The mere enumeration of these subjects, as insulated objects of study, shews, indeed, in a very forcible manner, how distant we are from that period, when a solid basis shall be laid for the construction of a system of medical logic. The separating the study of the anatomical formation of the body from that of its functions, and the functions in the state of health from those in that of disease; this, and all our histories of a multitude of individual phenomena as such, may have been useful, and indeed necessary, in a subject comprising such a multitude of various ideas: it is analogous to what has been done, apparently with benefit, in other and more confined sciences; but even though the distinct histories of each of those subjects were rendered perfect, a system of logic could not be formed, until some mighty genius shall arise, who has the awful power of mind to comprehend the whole at one view; to trace, at the same time, the series and order of the phenomena, and to determine their relations under every variety of known circumstances. But, when may such a genius be expected to appear? The history of man shows that nations have risen, flourished, and fallen, without an ARISTOTLE having been produced.

The hope, then, that a system of medical logic will be ever

instituted, must be but very faint; or, at least, the probability of its realization must be assigned to far distant ages. This only exists for the present generation,—to proceed steadily in the true road for the acquisition of knowledge, which we have at length happily discerned. We shall thus have the gratification of employing our talents in a manner the most immediately beneficial to human nature; and we shall at the same time be collecting materials adapted for the construction of an edifice, that would constitute the most glorious and useful work ever executed by man.

Such are our opinions respecting medical logic in the most comprehensive view; but, while the study of the science of medicine is parted into the divisions we have enumerated, it may perhaps be allowable to consider the development of the principles of each of those divisions in an isolated state, as well as it can be effected, as the logic of those divisions: and this appears to be the opinion of Sir GILBERT BLANE; for, if the work we are about to take into particular consideration may with propriety be termed "*Elements of Medical Logic*," it can only be so in the partial manner we have just described.

In an analytical report of a work of this kind, the most clear and useful method would be, first, to give a general scope of the author's design, and then to adduce the particular illustration of his principles; but, for reasons which will be hereafter apparent, we must on this occasion commence with particulars, following the author step by step in his own course.

Sir Gilbert Blane commences the introduction to this work with a definition of medicine: he says,

"As medicine has for its object the preservation and restoration of health, it comes under the definition of an ART,—a term, the import of which consists in the adaptation of means to ends. These means must be derived from the previous knowledge of the changes producible by them, whether as corporeal agents constituting physical causes, or as affections of the mind constituting moral causes."

This we consider to be only a partial view of medicine, using that term in the comprehensive sense in which it is generally received, and in which the author has himself employed it, making it to comprise physiology and pathology, as well as

therapeutics. That the two former are objects of science, must, we think, be admitted. To show this, let us consider the case of a man who has a portion of the integuments of the skull separated from that bone by a sharp-cutting instrument, without any forcible concussion of the subjacent parts: some days afterwards symptoms of disorder of the functions of the brain appear; the man soon dies; dissection after death shews signs of a diseased, and perhaps gangrenous, state of the membrane lining the skull internally, corresponding in situation with that portion of it from which the external integuments had been removed. The description of these things constitute only natural history, and the knowledge of their occurrence cannot properly be termed science, in the received meaning of that term; but, the explanation of the relation of one of these things to the other,—that is, the explanation of the causes of the results, by shewing that a due circulation of fluids in a part is necessary for its vitality; that the internal membrane of the skull is chiefly dependent on the external membrane for the supply of those fluids, which is effected by the immediate transmission of vessels through that bone from one surface to the other; and, consequently, that the separation of the external integuments from the skull, by depriving the internal membrane of the fluids necessary for its vitality, must be followed by the death of the latter. This constitutes science, just as certainly as the explaining why, in a right-angle triangle, the square of its hypotenuse is equal to the square of the two other sides. And, though the object of this knowledge is the application of medicinal agents to the cure of disease, yet the faculty of using those agents with precision cannot be acquired without this kind of knowledge. For example, a person has a disease of the hip-joint, but no uneasiness is felt in the region of that part; the pain is confined to the knee: the practitioner, following the indications of art solely, would here apply his remedy to the *apparent* seat of the disease in vain. Science here becomes necessary, as well as art. We think they cannot be separated in a general and comprehensive definition of medicine.

Sir Gilbert Blane next adduces a statement of what he considers to constitute reason: he says,

“The most precise criterion that can be fixed upon for dis-

tinguishing rational beings from brutes, is the faculty of adapting means to ends; and there is perhaps no operation to which the term *reason* is so appropriately applicable."

This proposition respecting reason is, we believe, generally acknowledged by philosophers; especially since the principles of it were so well displayed by MALLEBRANCHE, who shews, in a very plausible manner, that *the difference between simple perception, judgment, and reason, consists in this,—that, by simple perception, the understanding perceives a thing without relation to any other thing; that, by judgment, it perceives the relations existing between one or more things; and that, in reasoning, it perceives the relations perceived by the judgment:* it is the knowledge of these relations that constitutes the faculty of adapting means to ends. But the denial of this faculty to brutes, is a point in which many great modern, as well as ancient, philosophers, do not agree with the author. We shall pass over this question, of course, as not relating to medical logic.

The faculty of applying means to ends in the art of medicine, is a sort of knowledge, that "has, to some persons of a sceptical turn of mind," the author observes, "appeared so unattainable, as not to be worth prosecuting, and they raise the previous question, *an datur ars medicinæ?*" The author argues in favour of its validity, from the actions of the brute creation, and of savages, when suffering disease: he remarks, that savages but rarely die of old age, though their maladies are fewer and less essentially severe, than those of cultivated nations; he points out also, as an evidence of the reality of the medical art, the undoubted beneficial control it exerts over many diseases. What, too, would be the use of various plants, except as medicines? he inquires; and, finally, he says, to deny it, is to arraign the benevolence of nature, in subjecting man to the calamities of disease, without providing means for their relief. He concludes the introduction with observing, that it is his intention, "with unfeigned diffidence and humility, to endeavour to point out, in what medical truth consists; what are the difficulties that have obstructed its progress; and what the means of obviating them."

The preliminary observations to the body of the work commence with a remark, that it is the knowledge of the reciprocal

relations of cause and effect, and thence the faculty of adapting means to ends, and the just application of such as we can command, which constitute skill and judgment in the cure of disease, as well as in the other arts of civilized life. "These agencies," Sir Gilbert Blane continues to state, "are ascertained by observation and experiment: by the former we may be said to listen to nature, by the latter to interrogate her." He conceives that there exists a certain relation between the structure of the body and its qualities with the surrounding natural objects and phenomena. "Every reflecting mind," the author says, "must be struck with the admirable correspondence of the structure of the living body as a whole, and of the senses and functions in detail, in relation to external nature; such as the adaptation of the whole frame to the laws of gravitation, and of the eye and ear to the properties of light and air."

The author carries this sentiment further: he says there is a similar relation existing between the constitution of the mind and the laws of nature; the most essential attributes of these being the *constancy* of their operation.

"Now," he continues, "the human mind has as evident a relation to this constancy of the laws of nature, as the senses have to their respective elements; for, from the earliest period of life, there is, previous to all experience, a most unbounded confidence in the present and future constancy of events, manifested in all the actions and attainments of practical life. The belief that the sun will continue to rise every morning; that all bodies will continue to gravitate to the earth; that the human beings around us exist, feel, and think, as we do, may be quoted as examples of this untaught knowledge."

If this opinion be correct, the fable of the Sybarites, who blinded themselves in the darkness of the first night, believing their eyes would in future be devoid of use, must be considered an idle invention, instead of being, as the doctrine of some great philosophers would lead us to interpret it, a symbolic illustration of a metaphysical truth.

Sir Gilbert Blane, proceeding in a train of reasoning on the same principles, says,

"But this is not all, nor the most important coincidence of the frame of the mind with the established course of nature. In

all the effects produced by the action of external bodies on each other, and on our own bodies, there is a rapid and instinctive connexion established between cause and effect, in virtue of that part of the structure of the mind by which it is made susceptible of habit and association, particularly in early life. So that, not only every organ and function of the body, but every faculty of the mind, is co-relative with, or represents and reflects, as it were, the elements and laws of universal nature.”*

This doctrine was first advanced by the author in a discourse read to the Speculative Society of Edinburgh, in the year 1771; and, as must be obvious, it is in opposition to that which states custom to be the only source of our ideas of cause and effect. It was against Mr. Hume, the most forcible advocate of that doctrine, that Sir Gilbert Blane especially made this attack. We shall not presume to adduce our opinion on a question that still divides many great metaphysicians; but we must remark, that we think Sir Gilbert Blane, in the following paragraph, rather favours, than controverts, the notions he opposes. He says,

“ These confident expectations of the future, could never have been discovered by reasonings *à priori*; inasmuch as we know nothing of the tie which connects cause and effect: *nor can we form any anticipations of future events, but from the past experience of what may be called simple sequence.*”

These speculations must not be considered totally foreign to the proper subject of this work; because, by reasoning directly or analogically from what we have been taught by custom; or, according to the doctrine of Sir Gilbert Blane, by finding that, in imitating the sequences of nature, we can adapt means to ends, so as to bring about certain results; we acquire our first idea of *power*,—the want of accuracy in which, in the forcible expressions of the author, “ has given rise to those mischievous errors and inveterate prejudices, those numberless fallacies,

* “ See this sentiment more fully illustrated, in a lecture on Muscular Motion, read before the Royal Society, by Gilbert Blane, M. D. page 40. London, 1789. It is also most ingeniously and appositely alluded to, in Madame de Staël’s account of the German poetry, in her work entitled *De l’Allemagne*, page 334, vol. i. Paris, 1815.”

those nugatory and superstitious practices, with which the history of the world abounds, and which have proved sources of vice and misery, embittering and deforming human life and conduct."

We shall not pursue the subject farther than the author has here done; we think, with him, that some attention to it forms a proper introduction to medical logic: but it is from treatises expressly metaphysical that the knowledge of it must be acquired.

To determine correctly the relations of causes and effects, and from first and real principles to form rational inductions, is, as the author observes, the true object of philosophy. The living body being endowed with such a multitude of properties, and, consequently, its functions being so numerous and so variable under different circumstances, cannot, without extreme difficulty, be submitted to this rational research.

"But," Sir Gilbert continues to observe, "it is incumbent on those who allege that there are obstacles to physiological investigations, seemingly so insurmountable, to specify what they are.

"The author, therefore, submits to the profession the following enumeration of the properties peculiar to animated nature; meaning under it to describe all the ultimate facts, or primary elements, which form the ground-work of physiological and pathological science.

"These energies may be arranged as follows:

1. The Generative.
2. The Conservative.
3. The Temperative.
4. The Assimilative.
5. The Formative.
6. The Restorative.
7. The Motive.
8. The Sensitive.
9. The Sympathetic.

"This arrangement differs from any with which the author is acquainted, inasmuch as it is not founded on an enumeration of functions and organs, but on elements pervading and belonging to the whole animal system. It is meant to comprehend all the properties in which the essence of life consists, and

which characterise and distinguish it from inanimate matter on the one hand, and from a moral and intellectual nature on the other."

Had the author not expressly stated, that the above arrangement of the "primary elements of physiology" was not founded on an enumeration of functions, we could not have considered that he had intended to adduce any thing at all novel. Every system of physiology treats of the generation of the body, of the causes of its temperature, of the assimilation of the food, &c.; but these things are regarded as the results of functions.

We have exerted much painful thought in endeavouring to understand the author's precise meaning, and we are not certain that we have discerned it; for his illustrations of his principles rather embarrass than assist us. In the above paragraph, he speaks of certain energies existing in the body, of which we may suppose the functions usually designated by analogous substantive terms are the results: to assist the conception of the reader, we will term them so many *archæi*;—but on defining them, Sir Gilbert Blane on some occasions gives a substantive definition of the results of functions: thus, he says,

"The ASSIMILATIVE.—This consists in the chemical changes brought about in the decompositions and combinations effected by the power and processes peculiar to life."

"The TEMPERATIVE.—By this is meant that steady degree of heat with which all animals are endowed."

And, in another place, after noticing the hypotheses that have been advanced respecting it, he says,

"On weighing the whole evidence, it seems clear, that the heat of the living body is chiefly generated by the vital energies."

Thus, then, the temperative principle is *heat*; and heat is the result of the play of the vital *energies*. This word, in this place, can only be another term for *functions*.

"The MOTIVE.—By this is meant muscular action, in its most extensive sense."

"The SENSITIVE.—Sensation, being a simple idea, does not admit of definition, &c."

Others he defines, certainly, as active principles or primary

agents: let us take a view of his dissertations upon these. We transcribe the whole of what he advances respecting the first:

“The GENERATIVE.—It will not be disputed, that this primary energy of nature belongs purely and peculiarly to animal and vegetable life. Being emphatically named the mystery of nature, and being now admitted, by all correct physiologists, to be inexplicable, it only requires in this place to be barely enunciated. It may not, however, be without use here to hold out as a beacon to those who may still be disposed to waste their time and labour in attempting to overleap the stated boundaries of nature, the fruitless and absurd results they are likely to attain; for, what can be so extravagant and irrational, as that hypothesis which professes to explain generation, by supposing an infinite involution of embryos: *Obscura obscurioribus*. The doctrine of that most respectable physiologist, Dr. Blumenbach, who refers generation and growth to what he calls the *formative nisus*, is perfectly consistent with reason; inasmuch as it is to be considered rather as an exposition of facts than as a theory.”*

We shall follow the author's judicious advice, and not waste time in enquiries whether generation should be considered as the result of the play of certain functions of organized bodies, or as the work of an intelligent energy, an *archæus*, within organized bodies, moulding animal matter at its will; particularly as the subject of discussion is *medical logic*, on which nothing doubtful should be advanced,—nothing that is not an exposition of observed facts. Though, we must say, it is somewhat curious that we should be cautioned against enquiries on this subject, since they are likely to lead to absurd results, and immediately afterwards the doctrine of a physiologist designated as perfectly consistent with reason: whether as an exposition of facts or as a theory, does not signify; because, if one man has been able to discern facts respecting it, another may hope to do the same to a greater extent: and what is any knowledge but ideas of facts, or any theory but an exposition of them?

We proceed to the consideration of the author's illustrations of the other energies.

* “See D. I. F. Blumenbach *de Nisu Formativo*, Göttingen, 1787; and *Abhandlung über die Nutritionskraft*, St. Petersburg, 1780.”

“The CONSERVATIVE.—By this is meant that power by which the living body is prevented from running into putrefaction. According to the experiments of Dr. Alexander,* the range of temperature most favourable to the putrefaction of dead matter, being between 86° and 100° Fahrenheit, includes the usual standard of animal heat: there must therefore be some powerful energy in life itself, which counteracts this tendency to spontaneous decomposition.”

Reasoning on this must be hypothetical;—it is certainly hypothetical to suppose the existence of an abstract intelligent agent in organized bodies, for the sole purpose of preserving them from putrefaction: but it is a very probable hypothesis which attributes the preservation of living animal bodies from putrefaction, to the affinities between its particles constantly in play whilst its functions are performed. When these cease, those bodies then become subject to the more powerful affinities of the surrounding medium, which are chemical affinities, whilst the particles of the dead body are kept together only by the common gravitation, and the weak cohesion of soft substances.

The author, referring to this opinion, which was that, he says, of Dr. Alexander, and some other physiologists of that day, as it is that of many great physiologists of the present, too, should have been added, says, “It is quite inadequate to account for this striking phenomenon; and that there is an anti-septic power in life, independent of motion and the change of matter, is proved by the same principle of self-preservation being found in the quiescent state: for instance, in impregnated eggs and torpid animals.”

These things afford no support whatever to the author's hypothesis; for it is certain that torpid animals are *not in a quiescent state*; and it is very probable, if abstract reasoning alone can make any thing probable, that the egg is not so. The statement we have made respecting torpid animals, has been satisfactorily proved by the experiments of Professor MANGILI, of Padua: he has ascertained that respiration, and of course the circulation of the blood, are slowly carried on. This might have been well supposed; for, if there were no actions going on

† “See *Experimental Enquiry on the Causes of Putrid Diseases*. London, 1771.”

in them, how is the fat with which they abound before they become torpid absorbed, leaving them comparatively thin and emaciated when they come out of that state?

And, how can the egg be supposed to be in a quiescent state? It contains an organized mass: how can this have been formed without action; How is the chick subsequently developed, without action? It is more rational to suppose that a degree of action, insensible to our eyes, is constantly going on in the living egg, analogous to that by which its organization was developed,* than that it ever totally ceases for a time, and is then resumed. Besides, why is a living egg warmer in a certain temperature of the atmosphere than a dead one? and why is it not frozen in the same degree of heat? How can the *materia vitæ*, the existence of which the author favours, cause this difference in the living egg, in any other way than by producing *action*? We are at a loss to conceive how an organized mass, in an absolutely quiescent state, can be said to be influenced by any agent. If it is influenced by it, it must be acted on by it, and re-action must ensue. If it is not influenced by it, we must look for other reasons for the phenomena we have alluded to; and no reason can be so satisfactory, considering the subject in an abstract manner, as that there is a series of actions going on; and analogical reasoning from observed physiological facts shews the probability of this opinion; always bearing in mind that an egg is an organized animal body: and, with respect to the want of evidence to the senses of the existence of action, living plants stand in a similar relation to them.

The author adduces some other general arguments, which we consider as still less valid than those we have noticed, in favour of the existence of a *materia vitæ*, the original invention of which doctrine he attributes to JOHN HUNTER; and he expresses his surprise that "we meet with works on physiology, some of them even professing to be complete systems, in which this fundamental law of life is not alluded to."

Here we do not comprehend what the author means to signify: terming the existence of a principle, supposing it were not supposititious, a *law* of life, is using language in a manner so

* MALPIGHI says, the body of the chick is formed in an egg before the incubative heat is applied.

different from its common acceptation, that we cannot at all understand it.

We must make some remarks respecting the neglect of this doctrine of the existence of a *materia vitæ*. It is not admitted, either because it is considered that the cause of *life* cannot be regarded in an abstract manner, as distinct from organization; or because the admission of such a principle is, at least, contrary to the mode of philosophising inculcated by BACON.

We here pass over the *assimilative* and *temperative* energies, because, as we have already shewn, the author defines them, substantively, *heat* and *assimilation*, and states these to be results of the *functions of the SYSTEM*.

“The FORMATIVE.—This may be called also the organizing or plastic. It has not usually been stated as a principle distinct from the last, the *assimilative*. The slightest reflection, however, must evince that it is quite a separate act of nature, and as different from the *assimilative* as the construction of an edifice is from the preparation and collection of its materials.”

It is by this, the author says, that the procession of growth and repair are effected; and, as a subject for wonder, “with such harmony on both sides the body, as to produce that correspondence and symmetry which we behold!”

“This is a subject,” the author continues, “the nature of which eludes the keenest research, and overwhelms the mind of man with astonishment and despair; from which it can find no refuge, but in resting on it as an ultimate fact, and referring the whole to supreme intelligence.”

• In the latter sentiment we most cordially concur, but not in the former of those contained in the foregoing paragraph. We have, happily for our mental comfort, ceased to feel despair at not being able to discover the proximate cause of any natural phenomenon; but we know what it is to experience this suffering, and we yet remember it with pain. In early life, before we had studied the works of BACON, or become acquainted with the disputes of the Academic sect, as may well be supposed, we endeavoured to discover the cause of apparently one of the most simple of natural phenomena,—the passing of a ball from the hand of a person in a direction different from that which it would take when solely influenced by the laws of gravi-

tation. Our books told us that *momentum* was given to it. But what is *momentum*? It is something material, or it is immaterial; and how can it act on the ball after this has left the hand? We were indeed "overwhelmed with astonishment and despair." The hints these remarks convey may prove useful to some of our younger readers.

Returning to the author's dissertation on the formative energy, we find him remark, in continuation from the paragraph last transcribed, that,

"Should any one attempt to scan it further, by ascending higher in the scale of natural causes, (higher than supreme intelligence?) he will either find himself baffled, or will be in hazard of falling into some extravagance; such as that of Van Helmont, who held that there was in living beings an intelligent principle, which he called *Archæus*; presiding over, and directing, the secret movements of the animal machine; or of Stahl, who referred it to the rational soul."

We revere VAN HELMONT; and regret to see his opinions marked with the stigma of blame, without their merits being at the same time designated. His accurate and penetrative views led him to discern that there must be an active and powerful influence, seated about the region of the stomach, distinct from the other chief influences regulating the functions of the system; that is, distinct from the brain and spinal marrow. The functions of the ganglionic system of nerves had not then been determined by experiments; this was not effected until nearly a century after, by JAMES JOHNSTONE: but Van Helmont had in a manner anticipated the discovery; and the making that influence consist in an intelligent immaterial principle, was, with a few chemical absurdities, as Sir Gilbert Blane indeed observes, the error of his age. Put the ganglionic system of nerves in the place of his *archæus*, and his works must be perused with benefit and delight. We proceed with the transcription of another paragraph:

"The proper function of the formative faculty, is growth and repair. The long and universally received mode of conceiving the progress of growth, was that of a constant accretion of organic matter, giving additional length and breadth to the parts nourished. But it is evident that this would render the preservation of shape utterly incompatible with the en-

largement of dimension; and it was first clearly demonstrated by Mr. John Hunter, that the only process by which the growth of solid parts, particularly bones, could be carried on, was by a constant removal and replacement of particles."

Hence we must correct a little error in the history of physiological opinions. It may be difficult to determine when the nature of this process was first demonstrated, because analogous notions were advanced by the ancients; but it is certain that it was well developed long before the time of John Hunter. The story of the Dutch advocate, who saved his client from the sentence of the law for having committed murder nine years previously, by bringing distinguished physiologists to state their opinions that the human body was totally renewed in less than seven years, and therefore, as he showed, his client could no longer be the same man, is very ancient. However, this doctrine is fully and accurately displayed by BORDEU; and we particularly notice this, because we can at the same time refer to an author who dwells with much earnestness on some pathological notions, which Sir Gilbert Blane also advances as worthy of consideration: that there seems to be particular outlets for particular species of *effete matter*. Bordeu has perhaps carried this doctrine to excess, in his endeavours to rescue the physiology of the human body from the dominion of the chemists and mechanists, in that curious specimen of fervid discussion, his *Analyse Medicinale du Sang*.

On the *restorative energy*, the author observes, "it is well remarked by Dr. Gregory,* that it carries in itself the means of repairing the injuries and disorders incident to it." The chief illustrations of this which the author adduces, are the phenomena attending on sleep; several very curious and interesting facts respecting which he adduces; and its agency, as the *vis medicatrix naturæ*.

Whether the phenomena last alluded to depend on an abstract intelligent principle, or are results of the common laws of the animal economy, is a question likely to be a subject of scholastic disputation for a long period to come. We shall certainly not attempt to discuss it, whilst the works of BOYLE and GLISSON are in existence.

* "See *Conspectus Medicinæ Theoreticæ*, vol. i. p. 5."

The *motive*, the *sensitive*, and the *sympathetic* energies, we shall here pass over; because, as we have already observed respecting the two former, the author defines them as results of the functions of the system.

Here we arrive at the termination of the first section of this work; and we repeat, we are by no means certain that we have seized the precise meaning of the author respecting the "ultimate facts, or primary elements, which form the groundwork of physiological and pathological science," not founded on an enumeration of functions. We have some conception that he may intend, by these energies, only to designate phenomena resulting from the exercise of the functions, but which are not themselves the functions: in other words, that these are an analysis of the functions. But then, his express language does not authorise such a supposition; on the contrary, it, with regard to some of them, designates those energies as species of abstract intelligent agents existing in the animal body; each having its peculiar office, and exerting its influence on animal matter in virtue of its will, if it may be so expressed. And, if by these energies it is intended, as we have supposed, to designate an analysis of the functions, then there is nothing whatever of novelty in what the author has adduced, as far as relates to *principles*; though his illustrations of them, regarded in the point of view last alluded to, are often original and ingenious.

No work ever passed our critical examination that we have been so desirous to press on the perusal of our readers, as the one before us. The reason of this must be too obvious to require explanation.

There can be no question more interesting to the young physician of liberal acquirements and a reflective disposition of mind, than that relating to the importance of physiological and other scientific knowledge, in its application to medicine as an art. The history of the revolutions of medical opinions in different ages, and of their diversity at the present period, will rather add to his embarrassment than lead him to any determinate mode of conduct.

The method of HIPPOCRATES, so generally approved by experienced physicians, he will find to have been strictly enlighten-

ed empiricism. Many things, however, tend to make him not regard this in a favourable manner: he is unwilling to believe that the greater part of his laborious scholastic studies can be of so little utility to him as that method would inculcate; and he turns with eagerness to the opinions of the dogmatists, so flattering to his own inclinations. But let him pass in review the most brilliant systems of the methodic sect, whose precepts they have followed, and contemplate the results of the application of those of ERASISTRATUS, GALEN, PARACELSUS, BOERHAAVE, HOFFMANN, CULLEN, and BROWN; and he will be in doubt whether the benefits some of those have conferred on human nature, have not been counterbalanced by the ills that have emanated from others. The doctrines of another sect, especially if it be disposed to metaphysical reflection, will then appear in the most glowing colours. Its votaries will tell him, that the human body, which possesses the power of its own preservation;—of appropriating to its use aliment and air;—of resisting the destructive influence of various external agents, and of re-producing parts that have been destroyed by others;—may be well supposed to be able, in most instances, to restore, without the aid of art, the harmony of its actions, when accidental circumstances have thrown them into disorder. To observe the progress of the actions necessary to effect this through their different revolutions, without attempting to interfere with them when they proceed in a proper manner; and to regulate them, if they casually deviate from it, by such means as experience may have shown to be successful in similar cases, is, then, considered to be the only proper conduct of the true physician. But, there is only one step from this to absolute Pyrrhonism; the gloomy and repulsive regards of which will probably drive him from the path of the *Naturists*, although they have been adorned by almost the finest genius that ever walked in the regions of medical science. The *Eclectics* will point out the course of SYDENHAM, BARBEIRAC, and BAGLIVI, to his view; but he will sometimes be obliged to turn aside from them, and will then, notwithstanding the assurances of his advisers, often find himself in the midst of a mere chaos, constituted of the dispersed ruins of other systems.

Has not medical knowledge, then, attained such a state as to

admit of some conclusions on this subject, that may serve as general principles for the conduct of the young physician? This interesting question is discussed in the second section of the work before us.

In order to preserve a connected view of the subjects of this treatise, we should advert to the physiological dissertations that have already engaged our attention; or, from the want of any application of them, it might appear that our analysis had not preserved an unbroken train. The following paragraph will exculpate us in this respect, though it will probably lead the reader to criminate us in others; since we have not been able to discern the peculiar view to which the author therein alludes:

“The author has thus endeavoured to enumerate and define the primary elements, or ultimate facts, belonging exclusively to animated nature. In an attempt which is new,—in a subject, of which he has taken a view peculiar to himself,—he does not dare to think that he has attained any thing like perfection. It is evident, however, that it is only by following out an analytical scheme of this kind, that a foundation can be laid for the genuine principles of theoretical medicine; for the elements of disease can only be expounded by a thorough knowledge of the elements of life and health. But it is not his intention now to apply it synthetically, by building upon it any system of nosology, physiology, or pathology; far less to bewilder himself or his readers, by agitating the question, whether life consists in the play of those principles, or if it is something superinduced on them. His sole object, in this analysis, is to convey an adequate conception of the great difficulties which those have to encounter, who would found practical medicine on a knowledge of the animal economy; and to bespeak a liberal indulgence for the errors of those, who, in attempting this, have had to grope and wander in more dark and intricate mazes than what has fallen to the lot of any [other] class of enquirers into the various departments of nature.”

We repeat, we have not been able to discern the author's peculiar view in the preceding dissertation, either as relates to physiological principles or the pathological application of them; except the following remark, relating to the *restorative energy*,

may be considered to be a glimpse of it. We retrace our steps to adduce it.

“If one of the morbid poisons exciting fever should assail life by attacking one of its fundamental principles, the generating power of heat, this principle may re-act with such violence as to make it one of the main objects to repress it, either by internal remedies, or by the external application of cold; and the converse of this will happen, should the re-action be too feeble. This is not to be considered as an adopted theory, but merely a matter of hypothetical illustration.”

The reader will, we hope, excuse our making any remarks on this notion, and not consider that, in avoiding it, we shrink from the performance of our duty. The author promises to advert to this subject under another head; but we have not been able to discover where he has done this, unless the query, whether the *determination* to particular organs may not be considered “as a function by itself,” dependant on the motive energy, may be thus regarded.

We now return to the discussion on the utility of anatomical and physiological knowledge in the practice of medicine. The author first adverts to the errors that have arisen from the attempts made to explain the functions of animal bodies by the application of chemical and mechanical laws; and then takes a partial view of the circumstances that in different ages have either favoured, or retarded, the progress of practical medicine. In the rude state of society, he remarks, this seems to have been more indebted to those who discovered active and useful medicines, than to the labours of the learned. He passes the tribute of praise to DEMOCRITUS and HIPPOCRATES; and to ARISTOTLE, as a “most diligent observer of nature and collector of facts;” but, he continues, the syllogistic logic of this philosopher soon usurped the place of all literary and scientific pursuits. Yet Sir Gilbert Blane acknowledges, that, “in those dark times, [the middle ages,] it was better than no knowledge at all; and I am one of those who are of opinion, that this logic, though it affords little or no assistance in the discovery of practical truths, and the interpretation of nature, *is yet an excellent discipline of the mind, tending to give precision to language and thoughts;* more, indeed, in moral science and literature, than in the phy-

sical and exact sciences; *and that some knowledge of it can hardly be dispensed with in a liberal education.* I am thankful that it made part of the academical education which I received."

We have been particularly pleased with the sentiments in the above paragraph which we have taken the liberty to transcribe in italic characters, and wish Sir Gilbert Blane had expressed his opinion respecting the mathematics in a similar point of view: we think he must equally approve of the study of them; and they are now strangely neglected by physicians in general. MEAD said, he hoped to find this species of knowledge become thought as necessary to a physician as the acquisition of the Greek and Latin languages; but when the student reads his treatise on the Bite of the Tarantula Spider, and perceives the use that physician made of it, he must be disposed to treat his opinion on this subject with contempt: and the authority of Hippocrates and Galen will probably not be regarded with much more respect.

The merits of BACON are next alluded to, and appreciated: Sir Gilbert Blane, however, objects to his opinions respecting the utility of searching after first causes; but we never before saw them regarded in the manner in which he seems to view them.

Medicine, however, he continues to observe, did not avail itself of the light which broke upon science in general in the seventeenth century; but rather, from the long application of the physical sciences, became turned from its course, and oppressed with new errors. Even the knowledge of the circulation of the blood, "by its perverted application, tended to corrupt and mislead, by a loose adoption of the principles of mechanical philosophy, so well laid down in that age by Galileo and others."

We cannot have a more forcible illustration of this, than the manner in which the determination of the fluids to particular parts was for a long period regarded, and is indeed at the present time, by many physicians. The powerful influence of the nervous system in regulating the circulation, the peculiar action of the capillary arteries, and the distinct circles, as it were, formed by particular organs, have by them been lost sight of; and,

consequently, with this, the true knowledge of the real nature and origin of numerous diseases.

The well-known errors of the mechanists are then brought into view, especially those of BOERHAAVE; though the author does not neglect to pay a just tribute to the talents and acquirements of that celebrated medical reformer. But here Sir Gilbert Blane proceeds in too rapid a manner; since he passes over unnoticed the true founder of the present system of physiology to arrive at CULLEN, who, he says, "first clearly marked, and defined, the principles of life, as distinguished from those of dead matter."

Let us indulge in some remarks on this subject. We shall apologise for these, and some other circumstances respecting our conduct in the review of this work, before we conclude.

GLISSON may be considered as the legitimate founder of the present doctrine of vitality. He announced that matter is not wholly *inert*, as almost all the moderns who followed the doctrines of PLATO had believed. He supposed it to be essentially active; and that the properties thus given to it by the Author of nature, were the cause of the phenomena we observe, not only in the great bodies of the universe, but also in chemical changes, and in the re-actions of organized bodies on external agents. This doctrine he applied in a particular manner to the functions of the animal economy; and gave the term *irritability* to the power which organs derive from the active properties of the matter of which they are composed.* LEIBNITZ and HOBBS supported these opinions; and GORTER afterwards extended the application of them to plants.†

It remained for HALLER fully to develop and illustrate these principles, in such a manner as to leave no doubt respecting their truth in the mind of any man who has well examined them, and who will "on reason build resolve." Nothing remained to be effected but the discovery of some of the more minute relations of the phenomena to which they give rise; and, since his labours, they have formed the basis of physiology. The theory thence formed was applied to pathology with the utmost precision by BORDEU, who flourished before CULLEN; though

* See his Treatise *De Ventriculo et Intestinis*; 12mo 1677.

† *Exercitationes Medicae Quatuor*; Amstelodamiz, 1737.

the latter, from his situation as a professor in one of the most celebrated schools of Europe at that period, gave it the *eclat* necessary to lead to its general adoption.

Many physicians have expressed doubts of the utility of this sort of knowledge to the art of medicine; and Sir Gilbert Blane thinks them worthy of so much attention, as to be made the subject of a formal examination. After some general reflections on this subject, he proposes the following query:

“Are we, then, to admit, that the greater part of what we have been taught at the schools of physic, and of what we have read, or may read, in books, is in no wise conducive to our practical improvement?”

Far from it, he replies,—for

“Though anatomy, physiology, and pathology, should be proved to be of little or no avail,—nay, of pernicious tendency, in the practice of physic, the acquisition of these branches of education is nevertheless indispensable, in order to be armed with antidotes against the influence of fallacious theories, and to obtain the guidance of true beacons, instead of false light. There is nothing better known to those who are conversant in medical practice, than that the most ignorant and shallow, those of the least learning,—nay, those of no learning at all,—are the most addicted to hypothetical reasoning, the most infected with presumption and conceit.

“The knowledge of nature in all its branches,” he also observes, “is an indispensable requisite in the cultivation of the mind. It is highly useful, were it only as a gymnastic exercise of the understanding, by that salutary discipline of the mental faculties, implied in the acquisition of habits of attention, and the practice of the reasoning powers. Besides, all arts and sciences have a bearing on each other: and the history and philosophy of animal life, is surely as necessary an accomplishment to a physician as any other branch of science or literature.”

It also, Sir Gilbert Blane says, tends to free the mind from superstition, which has exerted so much deleterious influence in the practice of medicine.

The concluding reply is somewhat more favourable to the cause of physiological knowledge, though much of its force

must be averted by the reflections contained in those which have preceded it.

“ It must be obvious to every reflecting mind, that those who have made themselves acquainted with the various organs and functions engaged in the animal economy, must have a great advantage in practice over the unlearned empiric, in discriminating the morbid affections from each other, and in varying accordingly the respective means of relief. For instance, a physiologist and anatomist, from his knowledge of the intimate nature of morbid affections, the difference of their seat, and other circumstances, is able to distinguish spasmodic from inflammatory pains; a distinction which would not readily occur to an uncultivated observer, but of the most vital importance in practice; for the remedies required for the relief and cure of a spasmodic pain in the stomach and bowels, demands a treatment not only different, but opposite, to that which proceeds from inflammation. It is only anatomical and physiological science that can give a practitioner a clear and vivid conception of these and other distinctions essential to the safe and efficient treatment of diseases.

“ Whatever doubts there may be with regard to the degree in which anatomy is useful in physic, there can be no doubt of it with regard to surgery, in which an accurate knowledge of the relative position and structure of organs is indispensable.”

“ But,” the author then observes, “ if the benefits derivable to medicine from physiological science are so limited, from what other and better source is improvement to arise? The answer is, from accurate observation: in other words, from enlightened empiricism.” Of this Sir Gilbert Blane gives a well-drawn and impressive character, though it is rendered a little abstruse by his peculiar opinions respecting the responsiveness of our mental faculties to the constitution of external nature. We transcribe from it the following passage, which contains some notions, that, to say the least of them, are ingenious, and certainly merit the attention of those who are disposed to philosophical reflection.

“ The avoiding of fire, of precipices, the collision of hard and pointed bodies, may be quoted as examples of this [the responsiveness of our mental faculties to the constitution of external

nature.] And what is called sagacity, in the adult ages of life, is a sort of approach to, or imitation of, this intuitive faculty; but, instead of being the immediate suggestion of nature, is founded on cultivation, which, by practice, learns to connect cause and effect, means and ends; an operation which, in well-turned minds, is performed with promptitude and precision, by interpreting fairly the appearances of nature, and stripping them of those adventitious fallacies which mislead ordinary minds. In order to attain this, there are required an appropriate natural capacity, the good fortune of not being beset with prejudices in early life, an habitual exercise in the observance of nature, a candid and ingenuous disposition, an ardent love of truth, an exalted sense of duty, a large store of facts in a correct and tenacious memory, the power of combining, comparing, and discriminating these, by an intuitive glance, in the moment of applying them to the practical end in view. This is what is understood by the term *tact* in English and French, and εἰσροχία in Greek; being the faculty by which practical facts are decided on, and is performed by an instantaneous, silent, and almost unconscious calculation and induction, to be met with only in minds at once happily constituted and highly cultivated."

We have now only to adduce the author's general conclusion, in order to complete our analysis of this part of the work. We shall give this in his own words.

"From all that has been said, we ought to be in some measure qualified to come to a decision on the celebrated question of the comparative merits of the empirical and dogmatical methods of cultivating physic. It seems pretty evident, that, if either method were employed exclusively, or carried to an extreme, the art of physic would suffer, both in its efficiency and its prospects of future improvement. It has clearly appeared, that, under such a complication of causes, influencing the operations of life, it would be utterly hopeless to decide any point purely and strictly *à priori*; and that it is absolutely necessary that experience be called in as an aid and a test to the inferences of theory. On the other hand, a blind empiricism would be found deficient, without that discriminative judgment, founded on an acquaintance with the laws of life, and without those

enlarged and correct views of general nature, by which the excess of credulity and scepticism is equally repressed."

Having thus considered the chief of the obstacles to the progress and improvement of medicine, and the errors and abuses of false or misapplied theory, the author proceeds to that of the rest: these he supposes to be, *the great diversity observable in the constitution of individuals; the difficulty of appreciating the efforts of nature, and of discriminating them from those of art; superstition; the ambiguity of language; and the fallacy of testimony.*

The varieties in the character of disease arising from an identical cause, and the diversity in the operation of certain remedies, exemplified particularly in that of purgatives, form the subjects for illustration of the first of the above series; and this section concludes with questioning whether it would be too much to affirm, that all the practical works in existence ought to be re-composed, in order to insert in them, for the benefit of mankind, and the credit of the profession of physic, the following qualifying words: "The practice recommended will be found to answer in a great majority of cases; but there are numerous exceptions to it, which it behoves every judicious and conscientious practitioner to bear in mind."

On the next subject the author makes some general remarks only; but they bear the character of the rest of the moral reflections comprised in his work, of which we shall hereafter speak. Of the influence of superstition, he alludes to several remarkable instances; as, the unresisting submission to the progress of the plague of the Mahomedans; the opposition to inoculation of the small-pox from religious prejudice; the belief in the powers of magic, amulets, &c. in the cure of diseases, many curious examples of which he has adduced from the works of Sir Theodore Mayerne; though, the author also remarks, superstitious practices ought not in every instance to be disregarded, since the rust of Telephus's spear, mentioned by Homer as a cure for the wound it had inflicted, was undoubtedly a useful remedy.

The ambiguity of language, is illustrated by the application of the terms *scurvy* and *yellow-fever*; and on the latter subject the author enters into an elaborate discussion, founded on observa-

tions deduced from very extensive researches; the chief object of which is to prove that what is properly yellow-fever, is a contagious disease. In force and precision, and methodic arrangement, his arguments cannot be excelled, and they must bring conviction to all who admit the data on which they are founded; but, we think, before this question can be decided in a manner that will be generally satisfactory, it must be first determined what is to be considered as yellow-fever; or else similar arguments to those used by Sir Gilbert Blane may be brought to show that that disease is not contagious. His data are derived from a division of the fevers of hot climates into three classes: the *endemic*, arising from marsh effluvia; the *pestilential* or *malignant* disease, or *typhus icterodes* of Sauvages and Cullen, from human effluvia; and the *sporadic*. The second, Sir Gilbert Blane states to be yellow-fever. Now, other physicians will assert, that it is the first which should be thus termed; and, for proof, refer to that which raged in the United States in 1794, 1797, and 1800, and especially at New-York in 1805, which was by general consent termed yellow-fever; and there can be but little or no doubt of its being caused by marsh effluvia. RUSH at last abandoned the notion that it was propagated by contagion, although at first an advocate of that doctrine; and few men have been more cautious in judgment than him, and certainly not many better qualified to form a correct decision.

The observations and remarks on the fallacy of testimony, are too multifarious to be even alluded to in a concise manner: we can only say, that the reader will find amongst them many useful hints and judicious reflections, particularly valuable at a time when there exists a sort of rage for acquiring what are called new facts; instead of a disposition to meditate on those which are already known, and which have been established by the testimony of successive ages.

Before we part with the reader on this occasion, we have some apologies to make, and some remarks to adduce, that we particularly desire to impress on his mind. The first relate to the extent to which this article has been prolonged, chiefly by the minuteness with which we have examined some parts of the work to which it relates, and the passages introduced being

somewhat adventitious to the subject under immediate consideration: we should add, too, any part of our examination which he may probably think bears a tone of undue severity. We request him to reflect on the interesting nature of the subject here discussed, and on the great importance it is to the welfare of our art, that no principles should be formally and systematically proposed as valid bases for our reasonings, that are not well defined in their character, and substantiated by indisputable arguments. For the rest, our desire to give a perspicuous view of the object of the work must serve as our excuse.

With respect to the question of the propriety of considering this work as a treatise on Medical Logic, even as far as relates to physiology, we have shown that we cannot form a determined judgment, generally, because we have not been able to comprehend the author's meaning on some occasions; though, on others, we have considered his opinions to be hypothetical, consequently, not proper for the principles of a system. It was from the difficulty just alluded to, that we were led to express so earnest a desire, on a former occasion, that the reader should peruse the work itself: otherwise, we fear, he may attribute that difficulty to dullness in us, rather than to real obscurity in the author. But we have other reasons for recommending this work to his attention. It has here been examined more expressly in its medical relations: we have, consequently, passed slightly over, or wholly neglected, what we believe to be the best and most useful parts of it,—the many good moral reflections it contains; we should add, too, many physiological observations, adduced as illustrations of given principles, which we could not apply in the manner the author has done, and therefore neglected to notice them. But it is the dispersed moral reflections that we have regarded with most pleasure: these form a series of traits of the medical character, calculated to exalt its dignity, in a scientific and moral point of view; and, when possessed, will render the physician really qualified for the practice of an art *Deorum immortalium inventioni consecrata*.

A History of the High Operation for the Stone, by incision above the Pubis; with Observations on the Advantages attending it; and an Account of the various Methods of Lithotomy, from the earliest Periods to the present Time. By J. C. CARPUE, F. R. S. &c. &c.

[From the London Medical Repository, for October, 1819.]

IN our last general retrospect of medical literature we made particular mention of an interesting little volume, by Mr. Kirby, entitled "Cases in Surgery;" and among other points of novelty contained in that work, it will be recollected we announced the author's avowed intention to resort, in his own practice, to the high operation for the purpose of extracting a calculus from the bladder, as soon as a suitable opportunity and subject should be presented to him. In relation to this particular, Mr. Kirby says, "Though the attempt to revive an operation, which the experience of its disadvantages and dangers was thought to have justly excluded from operative surgery, may subject its author to the charge of being desirous of innovation; still I cannot resign the conviction forced upon my mind by circumstances to be detailed hereafter, that the high method has been too hastily relinquished, and that a safe means of extracting a calculus from the urinary bladder was somewhat giddily laid aside, to make way for one requiring more dexterity and display, and, unquestionably, involving a more considerable risk."

Mr. K. was, doubtless, not aware, when he penned the above sentence, that a laboured defence of the same opinion was in the act of preparation by a Surgeon, who has attained so much eminence in his profession, as to entitle every thing which he may propose, either in the way of doctrine or practice, to the respectful attention of his brethren. The publication before us, as its title imports, is for the especial purpose of pointing out the advantages which, in many cases might accrue from the high operation for the stone, as practised at the present time by some of the best surgeons in France.

"In the year 1817, (says Mr. Carpue,) wishing to know the state of surgery at Paris, I made a journey to that city, was

much pleased with the cleanliness of its hospitals, their medical arrangements, and highly flattered by the friendly reception I received from the majority of the medical practitioners. The intention of my journey being known, Mr. Morand, of Rue St. Honoré, an eminent Apothecary, sent me a note, saying, if I wished to see the operation of lithotomy, a celebrated Surgeon, Dr. Souberbielle, would operate early the next morning, at the Hotel des Invalides, on a gentleman, aged sixty-four. Mr. Morand gave me a letter of introduction, addressed to Mr. Rebis, one of the surgeons of that magnificent establishment, who received me with great politeness, and did me the favour to introduce me to Mr. Ivan, Surgeon in chief. I had also the pleasure to meet Baron Percy and Dr. Chaussier. The patient, M. de Walville, entered the room with much composure, and ascended the table. Being placed as is usual in the operation for the stone, Dr. Souberbielle, having introduced a staff, made an incision in the perinæum, and into the membranous part of the urethra; he then introduced a director into the groove of the staff, which he withdrew: he now passed along the director, an instrument that had somewhat the form of a catheter, which was held by an assistant. It is not possible for any one to conceive my astonishment: I could not comprehend for what purpose this was introduced. The director was removed, and the instrument was suffered to remain in the bladder. The ligatures were taken from the feet of the patient, who was placed in a very different position. The operator made an incision in the integuments and fat three or four inches in length above the pubis. I now perceived that Dr. Souberbielle was about to perform the high operation. He proceeded with great coolness, and extracted a large stone, then another, another, and another; he now drew forth a large quantity of small calcareous particles: this rendered the operation tedious, which the patient bore with great courage. Baron Percy said, ‘Dr. Souberbielle, I think that you had better inject the bladder:’ the doctor answered, ‘there is no occasion, for I can feel every part of the bladder, and there is not a particle of calculus remaining.’ The operation being finished, the patient was put to bed. We retired into the adjoining apartment. Baron Percy said it was usual for the Attendant Surgeons, when they ap-

proved of an operation, to signify the same. On this he wrote the annexed paper, stating that Dr. Souberbielle had operated with great ability; that the stone consisted of variously sized particles, which, when collected, were about the size of a turkey's egg; and that the Professors and Doctors present, (the Surgeons in Paris rank with, and have the degree of Doctor,) were of opinion that the patient was in a state which afforded the greatest hopes of his recovery."

Mr. Carpue goes on to inform his readers that the patient speedily recovered, and that several other individuals who had been operated on in the same manner, some of them under unfavourable circumstances as to age, &c., were speedily and effectually relieved from their sufferings by this high operation thus performed by Dr. Souberbielle. So much impressed was Mr. C. with what he saw, that he, like Mr. Kirby, conceived an immediate bias in favour of this manner of performing lithotomy; and he therefore proposes the restoration of an operation with which he confesses himself hitherto to have been totally unacquainted, and which "had not been performed in London for a period of nearly a hundred years."

It will have been perceived by the reader, that the principal feature of novelty in Dr. Souberbielle's operation consisted in the introduction, by the perinæum, of an instrument, prior to the incision being made above the pubis; and this first step of the high operation performed in this manner does away, Mr. C. conceives, in a great measure, the objections which lie against it. Upon further prosecuting the investigation of this subject, Mr. Carpue, however, finds that the method above described, although to him novel, is not in fact new, but is the one suggested and employed by the celebrated Frère Côme, in order to obviate the inconveniences attendant upon injecting the bladder for distention.

The principal part of Mr. Carpue's volume which is now before us, is made up of a history of lithotomy; to follow which perhaps, even in abridgment, would not be very interesting to the generality of our readers. Suffice it to say, our author contends, that Franco, who was born at Turrière, in Provence, in the sixteenth century, was not only the actual inventor of the lateral operation, but "that the situation of the patient, his

treatment before and after the operation, the instruments used, the incisions made, were the same which are now in use." Mr. C. endeavours further to prove that Franco was only restrained by an apprehension of obloquy which might follow the proposal, from openly defending and practising the incision above the pubis.

We, however, pass over the preliminary matter of Mr. Carpue's book, as not so immediately bearing upon the main object of its publication, and proceed to extract the account which the author gives of Frère Côme's method of performing the operation in question.

"Frère Côme's book, which was published in the year 1779, is entitled, "*Nouvelle Méthode d'extraire la Pierre de la Vessie urinaire par dessus le Pubis, qu'on nomme vulgairement le Haut Appareil—dans l'un et l'autre sexe, sans le secours d'aucune fluide retenu ni forcé dans la vessie.*"

"Frère Côme quotes Douglas, Cheselden, Macgill, Thornhill, Bamber, and Heister.

"In this publication he gives a chapter on the necessity and advantage of the method called the High Operation; but he remarks, that it will require an experienced lithotomist to decide in what cases the operation is necessary.

"Frère Côme says, the operation should be performed where there is an affection of the perinæum or of the urethra, or the disease of parts contiguous to the bladder, or the largeness of the stone, or the weakness of the patient, who cannot bear the irritation of the extraction by the perinæum. In females, from his experience, he finds that when the urethra is dilated or cut, there is generally an incontinence of urine, which is as much to be dreaded as the stone itself. He says, that in females the High Operation is far superior to every other.

"If there is inflammation or thickening in the bladder, it will be impossible to distend the bladder with water; and in the female, the urethra being short, it will not be possible to retain the water injected into the bladder. These circumstances considered, as also the great pain occasioned by the injection of fluid into the bladder, and which was carried to such an extent as even to burst the bladder; that the urine, pus, &c. &c. were often effused in the cellular substance contiguous to the

bladder, produced frequently sinuses, lodgment of matter and urine, spasm, &c.

“These circumstances being considered, induced Frère Côme to make a number of experiments on the dead body, the result of which was the present mode of operation.

“FRÈRE CÔME’S OPERATION.

“A METHOD TO EXTRACT THE STONE FROM THE BLADDER,
WITHOUT THE INJECTION OF FLUID.

“*Situation of the Patient.*

“In the first part of this operation the patient is placed and secured as in the lateral operation; that is, his hands are tied to his feet; he is held by two assistants, one holding each thigh and foot, and a person supporting him behind.

“When the sonde-de-dard has been introduced into the bladder, it is to be held by the assistant, and the ligatures are to be taken from his hands and feet. The lower extremities are to be held and supported by assistants; the arms are to be brought behind the patient’s head, and to be also held by assistants. Pillows are to be placed under the patient’s head and shoulders.

A List of the Instruments invented by Frère Côme for the High Operation.

- “ 1. A staff.
- “ 2. A grooved director.
- “ 3. A sonde-de-dard.
- “ 4. A trois-quarts bistouri: this is a trocar, in which is contained a bistouri.
- “ 5. A curved probe-pointed bistouri.
- “ 6. A suspensor of the bladder.
- “ 7. A bistouri cachée.
- “ 8. Flexible gum catheter.
- “ 9. Forceps of different kinds.
- “ A staff is introduced into the bladder, which is to be held by an assistant; a small opening is made in the perinæum.
- “ A second incision is made in the membranous part of the

urethra, as close to the prostate as possible, on the groove of the staff; a finger is to be passed into the groove.

“ The operator takes a grooved director in the right hand, and introduces it into the groove of the staff. He now takes the staff from the assistant, bringing the handle downward, as in the common operation, and passed the director into the bladder.

“ The staff is now withdrawn.

“ The sonde-de-dard is introduced into the bladder upon the director, which is to be withdrawn. The ligatures are now removed.

“ An incision is now made through the integuments and fat, about three or four inches in length, in the direction of the linea alba from above downwards to the pubis.

“ A trocar, in which there is a concealed bistouri, is passed into the linea alba close to the pubis.

“ The concealed bistouri is pulled towards the navel, which cuts the lower part of the linea alba.

“ The operator takes a scalpel with a blunt point, and introduces it into the incision; holds the handle with his left hand, and the back of the blade with his right. This he pushes forward, and divides the linea alba.

“ The operator now takes the sonde-de-dard in his right hand, and pushes it upwards, by which means he elevates the bladder, and feels with his left hand for the instrument. Great care is to be taken that the sonde-de-dard is kept as near the pubis as possible, the bladder being elevated by the sonde-de-dard.

“ The assistant pushes the stilet, which is concealed in the cannula of the sonde-de-dard, through the upper part of the bladder. The operator takes hold of the stilet, in the anterior part of which is a groove or director.

“ He now takes a probe-pointed bistouri, and introduces it into the groove of this director, and makes an incision in that part of the bladder which is opposite the symphysis pubis. The stilet is now withdrawn. The index of the left hand is introduced into the bladder, which suspends it.

“ A suspensor of the bladder is introduced, and given to an assistant to hold. The operator now withdraws his finger from the bladder, the bladder being suspended by this instrument.

“If the incision is not sufficient for the extraction of the stone, a bistouri cachée is introduced into the wound, which is enlarged.

“A flexible gum catheter is to be introduced, and the stone extracted.”

When Mr. Carpue returned from a second visit to Paris, in the year 1818, he waited, he informs us, upon Sir Joseph Banks, in order to exhibit to him the apparatus for the high operation, and the cast of a large stone which had been extracted in this manner by Dr. Souberbielle. At Sir Joseph's he met his friend and old preceptor, Sir E. Home, who agreed immediately that such a calculus could not have been extracted by the lateral operation. In consequence Sir E. determined to have recourse to the high method on a boy who was waiting to be operated upon at St. George's Hospital. This soon took place; and Mr. Carpue was present, who tells us that the operation was much longer than it otherwise would have been, from the stone being in a cyst; yet still it was very short, compared with what the same operation would have proved in the lateral way. The operation lasted exactly seventeen minutes. The boy recovered, and is at this time in perfect health.

Upon the whole, then, Mr. Carpue concludes, that in by far the majority of cases, the operation which he witnessed at Paris, and which Sir E. Home so recently performed in this country, is far preferable to that commonly, and hitherto almost exclusively, employed by British surgeons; the great objections to which are, that we are working in the dark, with doubtful success, and lacerating and dividing parts to such an extent, and in such a manner, as often to be productive of fatal hæmorrhage and other serious consequences, one of the most common of which is wounding the rectum. The stone, he goes on to say, is more readily discovered by the high than the lateral method; and if it breaks, the particles can be extracted with more certainty: if too the stone be in a cyst, or situated above the prostate, it can with greater ease be found and extracted, without danger of including part of the bladder, or inducing a fistulous opening.

Mr. C. allows that the high operation cannot well be performed on a corpulent subject; that it is altogether out of the

question when there is scirrhus, or any other affection of the bladder, which should prevent the sonde-de-dard from raising the bladder above the pubis; but then, on the other hand the lateral method could never be had recourse to where a stricture of the urethra should prevent the passage of the staff, or where the calculus is of a certain magnitude. In these cases, he adds, the high operation *must* be performed, or the patient is doomed to linger out a life of wretchedness.

Although our extracts have already been copious, and the present article, by consequence, threatens to be presented to us in the printed sheets of a disproportionate magnitude, we cannot refrain from presenting to our readers, at full length, Mr. Carpue's recapitulation of the several steps of the high operation, as now practised.

“ A staff is introduced into the bladder.

“ First, an incision is made through the integuments of the perinæum, and a small incision into the membranous part of the urethra; a director is introduced into the bladder, upon the staff: the staff is withdrawn; the sonde-de-dard is introduced upon the director into the bladder: the director is now withdrawn; the sonde-de-dard is held by an assistant. An incision is then made, three or four inches in length, through the integuments of the abdomen. The trocar-bistouri is passed through the linea alba, close to the posterior part of the pubis. The concealed blade is opened, by means of which the lower part of the linea alba is divided. A probe-pointed bistouri is introduced, through the opening which had been made by the concealed bistouri, into the lower part of the linea alba, and the incision is continued by means of this instrument. The operator takes the sonde-de-dard from the assistant with his right hand, and pushes it forward, by which means he elevates the bladder above the pubis. The assistant now holds the sonde-de-dard, and the surgeon with his right hand pushes the stilet, which is contained in the cannula of the sonde-de-dard, through the superior and anterior part of the bladder; he takes hold of the end of the stilet with his left hand, and passes a probe-pointed bistouri along the groove, which is in the anterior part of the stilet, and makes an incision in the superior anterior part of the bladder. He passes the index finger of his

left hand into the bladder, by means of which he supports it. The stilet is withdrawn from the cannula of the sonde-de-dard, which is now lowered and held by an assistant; the operator introduces the suspensor of the bladder, which is held by an assistant. The stone is now to be withdrawn with the finger and thumb, which, if small, is done with great ease. If the bladder is large, a finger is introduced per rectum, by which the bladder is elevated, and the stone more readily found. If the stone should be in an excavation, and the bladder is not of a very large size, it may be discovered with the finger, by means of which the surgeon will know whether a scoop, or what kind of forceps is indicated. If the stone should be of such magnitude as is represented in the case of widow Donneri, Frère Côme's forceps must be used; if the stone should adhere to the bladder, or be contained in a cyst, the means used by M. Baseilhac and Sir Everard Home should be resorted to. When the stone has been extracted, Dr. Souberbielle introduces a silver wire through the cannula of the sonde-de-dard, and passes it through the wound made in the linea alba; this is held while the sonde-de-dard is withdrawn; a flexible gum catheter is now passed into the bladder, through the wound in the membranous part of the urethra, by means of this wire; the wire is now withdrawn; the catheter is confined in this situation, by means of tapes passed round the thighs and pelvis of the patient; a bladder is tied to it to receive the urine. Though this method of introducing the catheter is done in a very short time, yet I think that if the point of the catheter were introduced into the sonde-de-dard, the sonde-de-dard might be brought through the opening in the linea alba, and by this means the catheter might be conducted into the bladder with the greatest facility; or the cannula of the sonde-de-dard might be made of flexible gum, which, in place of silver, would contain the stilet; and this might remain in the bladder, and the lower part might be cut off, thus answering every purpose.

“ Method of dressing the Patient.

“ A piece of soft linen, half an inch wide, and six or eight inches long, is to be introduced, by means of a pair of forceps,

into the bottom of the bladder; the edges of the wound are to be covered with lint: to prevent the urine excoriating the parts, the linen is to be allowed to pass over the pubis on either side, and by this means the portion of the urine which is not carried away by the catheter will be carried off by means of this linen.*

“Lint and light dressings are to be applied to the wound, and a bandage passed round the abdomen.

“Great care is to be taken that the catheter is kept open; a stilet should occasionally be passed. Much attention should be paid to the subsequent dressings. Usually on the third day the linen may be taken from the bladder, as by that time the greatest part of the urine will pass by the catheter. By this time the wound usually suppurates. Adhesive plaster may be applied, in order that the divided parts may be brought into contact. In the course of twenty years’ practice, I have invariably found, that the after treatment of the patient is not of less importance to his life, than is the operation itself.”

Time and comparative success must, of course, be the umpires in the great question now agitated; but we cannot, we confess, help feeling that much pain and risk might, in many cases, be saved by at least the occasional adoption of that method of operating, which the book now before us so highly recommends.

Our author concludes his volume by a few remarks on the three methods of puncturing the bladder; viz. above the pubis; by the perinæum; and per rectum: the last is, in by far the majority of cases, the one to be preferred: but Mr. Carpue states, that the only part where the bladder can be punctured with safety, per rectum, is a very short space beyond the prostate, between the vasa deferentia. This position is illustrated by an engraving. The present volume also contains etchings of some of the instruments formerly and at present used in lithotomy.

* “If you place a piece of linen in the bottom of a cup filled with water, and let the ends rest upon a plate, in three or four hours the whole of the water will be in the plate.”

An Inquiry into the Nature and Properties of the Blood, as existent in Health and Disease. By C. TURNER THACKRAH, M. Roy. Col. Surg. and Lc. Soc. Aph. 8vo, pp. 132. London.

[From the London Medical and Physical Journal, for October, 1819.]

This work has been produced, the author states in a preface, in consequence of Mr. A. Cooper having offered to the gentlemen educated at the school of Guy and St. Thomas, a prize for the best dissertation on the blood. "I instituted some experiments on the subject," he continues to observe, "and stated the results which my observations afforded. This essay being so fortunate as to obtain the prize, I have been induced to present it to the public: and, since the late period at which I heard of Mr. Cooper's proposal prevented the Inquiry's comprehending, in the first instance, some points of importance, I have, during the last year, been endeavouring to supply the deficiency."

We have not often perused a work to which we could give more unqualified approbation. Extensive and sound erudition, great accuracy in all the necessary observations and experiments, comprehensive views, and a particular clearness of exposition, mark the character of the work; and, what is of especial importance, and which renders it interesting beyond the merely physiological relations of the subject, the author has seized every opportunity of making researches tending to the improvement of the practice of medicine; and he has adduced from them several novel and highly useful observations, which he has illustrated by his judicious remarks.

The circumstances of this being a successful prize-dissertation, and the subject being one respecting which a new and comprehensive treatise was so much required, deduced from researches after facts, uninfluenced by any preconceived hypothesis, will, we feel confident, induce medical practitioners in general to become possessed of the work: we shall therefore pass over in a rapid manner the chief part of it, that we may notice more at length that which is especially applicable to the

practice of medicine. An abstract of this may prove a fertile source of reference, on various important occasions.

After some general observations on the origin, character, and uses of the blood, the author notices its most remarkable physical characteristics: and, first, its coagulation on removal from the body, and its change from a homogeneous fluid to the division into the serum and crassamentum. Its proportion to the weight of the body, is next considered. "Keil," the author observes, "estimated it at one hundred pounds; others do not believe it to exceed eight pounds. Haller computes it at ten; Young at forty; and, in Cooper's lectures, its proportion to the solids of the body, is considered as one to sixteen or twenty." Eight or ten pounds, he justly remarks, must be concluded too small a quantity for the supply of the numerous vessels of the body, occupying so extensive a space; and, indeed, it seems to be shown by the quantity lost within a few days, spontaneously or abstracted by art, being frequently greater than that, without the destruction of life.

The question of the vitality of the blood, is next briefly discussed; which the author is disposed to decide in the negative. The arguments he adduces to this effect, are novel and ingenious. The advocates of the affirmative are, we believe, becoming daily less numerous.

On the subject of the chemical qualities of the blood, no original observations are adduced.

The next chapter is on the peculiarities of the blood in different classes of animals. The author first confirms the statement of former physiologists, that there is but little variation of its character in those of the higher orders. The *relative quantity* was found, generally speaking, to be less in birds, fishes, and the weaker animals, than in the larger and more muscular. No uniform disparity was found to exist in the *relative quantities of the serum and crassamentum*; but, the author states, "it appears probable, that a more complete examination would prove the crassamentum to bear a proportion to the strength and ferocity of the animal." With respect to the *periods of coagulation*, "a general inference might be drawn, that coagulation commences sooner in small and weak animals, than in the large and strong." Its *temperature* in the

horse, when flowing from a vessel, is 97° ; in the ox, 100° — 101° ; in the sheep, 102° — 103° ; and in the duck, 107° . This, the author remarks, is conformable to the statement of Braun. No considerable variety exists in its *chemical qualities*; excepting that, according to Berzelius, that of the ox contains a smaller quantity of saline matter, and a larger proportion of azote, than that of man. Its *specific gravity* is nearly uniform. “The *red globules* exist principally in the more perfect animals: in the mammalia and birds, partly in fishes; but not generally in reptiles, insects, and worms. In some creatures, coloured blood is found in the vessels near the heart, while the rest of the body is supplied only with a serous fluid.” “Of those creatures which want the red particles, most have *white globules*; but, in the lowest orders, even these cannot be discerned by the microscope.” The blood of some animals is found, while circulating, to contain air-bubbles. In the land and sea tortoises, in some fish, in the hedge-hog, and the viper, this appearance has been asserted by respectable writers.—MORGAGNI, Ep. v. 22.

The *coagulation* of the blood is then particularly discussed. On this subject we find some original observations, possessing a considerable degree of interest. The author first notices the effects of chemical agents mingled with it on removal from the body. Medicines, internally administered, had no apparent influence on it. Stupefaction from opium does not affect it. It is induced readily, in proportion to its paucity. Agitation retards it. With respect to temperature, it concretes soonest at from 100° — 120° ; next at from 40° — 50° ; and last, and with a greater disparity, in that of from 60° to 90° . And the author thinks it worthy of remark, that the serum is most readily and copiously effused in the higher temperatures; and this, he believes, in regular gradation.

A subject which particularly engaged the author's attention, is the *comparative periods of coagulation, as influenced by the strength or weakness of the vascular action*. From various and repeated experiments, under different circumstances, on the blood of oxen, sheep, horses, dogs, and swine, it appears that the *blood coagulates slowly, in regular proportion to the tonic state, or that condition of the system in which the vital*

powers are strongest. It also appeared, that coagulation occurred soonest in *venous blood*.

The *causes of the blood's coagulation* constitute one of the most interesting parts of the enquiry; and it has been investigated by Mr. Thackrah with care and attention proportionate to its importance. We shall only adduce his conclusion respecting them; remarking, at the same time, that his grounds for it appear to be satisfactory, though we are unable to form any rational explanation of this phenomenon. It is "that the vital or nervous influence, is the source of the blood's fluidity; and its loss, the cause of coagulation." But this conclusion, it must be observed, applies to the vessels in which it is contained, not to the fluid itself. Indeed, the series of experiments that led the author to form this conclusion, appear to us to furnish very powerful arguments *against* the doctrine of the vitality of the blood.

The above opinion of Mr. Thackrah, we should remark, is similar to one advanced several years since by Mr. Charles Bell, in his *Anatomical Lectures*; and was then, we believe, considered to be merely visionary by many of his auditors. But the experiments of Mr. Thackrah seem almost to demonstrate its truth.

We now arrive at the chapter treating of the *changes produced by disease*. After some reflections on the degree of importance and certainty attached to them, the author treats of the *quantity* of the blood. This is a subject on which only probable suppositions can be formed, but they are in favour of its diminution under certain circumstances. The *colour* of the blood is frequently altered in a remarkable manner. Long continued hæmorrhages render it pale. Sometimes, chiefly observed in robust and muscular men, the venous blood is of a dirty-red colour; and it is also occasionally found to be florid. A darker, or livid hue of it is stated by authors to have been witnessed in *malignant* fevers. Several instances are related, or referred to, when it has been found so much lowered in *temperature*, as to be cold to the touch; and this without the patient apparently labouring under any severe disease. The relative rapidity of its *coagulation* has been already adverted to, and the cause of it explained.

"*The firmness of the coagulation of blood*," the author observes, "has been considered a distinctive mark of the tonic state of the system; its greater tenacity, a characteristic of inflammation; and its looseness, a sure proof of debility." Although these marks cannot wholly be relied on, yet his experiments have shown that they are in general indicative of the states of which they are said to be signs. Much difference in its appearance will arise from the size of the vessels in which it is received. "The fluid of blood received in a bason, is usually in greater proportion than that contained in a small cup; and, of course, the cake in the latter is looser than that of the former." And, as a general practical axiom, he says, "If, on the division of the coagulum, at the expiration of from eight to twenty-four hours, there ensue no considerable effusion of serum, and the crassamentum remain extraordinarily firm, I believe that further depletion is fully warranted." He next adduces the results of the experiments of Dr. Langrish, and the observations of Dr. George Fordyce, respecting the tenacity of the coagulum in different diseases; from which it appears, that in acute maladies it is generally inordinately dense.

"We frequently," the author proceeds to remark, "observe much benefit derived from bleeding, even when the crassamentum is soft and yielding; nor should we, in such cases, hesitate to repeat the depletion, if other circumstances indicate its propriety. Dr. Watt, in his cases of diabetes, remarks, that great advantage accrued from venesection, though the coagulum was loose and black; and that, on repeated evacuations of blood, the crassamentum became much firmer, and of a more natural hue."

Here, of course, blood-letting produced those changes, by removing the diseased state on which the alteration of the qualities of the blood depended; for such a condition of the blood is not common in diabetes. It is usual to find it present the sizzly coat in that disease.

The *proportion of serum and crassamentum*, is next considered. This is usually in a ratio *inverse* to the strength of the system. Two very interesting observations are adduced, which show that the *serum is relatively increased during the*

continuance of bleeding, and in a considerable degree in the space of a few minutes. Dr. Weatherhead suggested the following query to the author respecting this circumstance:

“Does not the abundance of serum in the last-drawn cup, arise from the immediate affect of the bleeding, in rousing the energies of nature to absorb serum from the different cavities, and thus occasion the fact you have remarked? If this be the true cause, it will likewise account for the benefit derived from, and authorise the detraction of blood in both sanguineous and serous extravasations, wherein the strength has not thereby been materially diminished. This absorption ought also to be more obvious in health, where nature acts unfettered by diseased associations.”

The circumstance must, Mr. Thackrah conceives, originate either in the cause Dr. Weatherhead suggests, or in the greater disposition to concretion which the blood assumes, when the system is reduced. “The latter opinion,” he continues, “is the least probable; for, if the increased proportion of fluid in the last received blood, arise merely from the speedy contraction of the crassamentum ejecting a greater quantity of serum than the first drawn, this substance would be firmer in the last than in the first.”

The *mechanists* would readily explain the above phenomenon by the *vacuum* produced in the vessels by the abstraction of blood; whilst the *animists* will perhaps concur in the opinion of Dr. Weatherhead. Others will seize the fact as an important indication for practice, without attempting to form any conclusion respecting its cause.

The author next adduces a statement of the relative quantities of the serum and crassamentum, in various forms and stages of disease; which is consonant with the general account already given.

We now arrive at another very interesting part of the subject,—the coat of yellowish size, which sometimes covers the surface of the blood. We cannot extend the limits of this analysis much further; and therefore, to avoid repeated particular remarks, we state, that the whole of the observations and experiments contained in this chapter, tend to show the truth and importance of the opinions of Bordeu, given in the last

number of this Journal. We hope to find them become the subject of discussion, and shall therefore at present briefly pass over this part of the enquiry.

Mr. Thackrah satisfactorily shows, that the *sizy coat does not arise from any particular tenuity of the blood*, as stated by Hewson; and this physiologist proved, that *increased gravity of the cruor was not* the cause. And though the author is disposed to infer, that the formation of the buff-coat in inflammatory diseases depends on the blood's indisposition to coagulate, such tardiness of concretion allowing the red particles to subside, yet he acknowledges that an observation of Dr. Whiting militates against this opinion. "He found a fibrous tunic near the *bottom* of the crassamentum in one case, and this phenomenon to be wholly absent in another, though the blood remained fluid from fifteen to thirty minutes." We cannot here pass over one circumstance that we have ourselves almost constantly observed, and in instances of which the works of every writer on inflammatory affections abound; which is, that the buffy coat is not witnessed in the first or second day of the disease, yet it afterwards appears, even though the severity of the malady has become much alleviated. The observations and experiments of Mr. Thackrah, in regard to this part of the subject, are not quite perfect; since he has not noticed the stage of the disease in which the blood forming the subject of them was drawn.

The author has also been led to doubt the truth of the opinion, attributing the buffy coat to *increased agitation of the blood*.

Mr. Thackrah adduces the following queries respecting it; the analogy of one of which to some opinions we have already noticed will be easily discerned.

"Does the additional quantity of fibrine, acquired by inflammatory blood, remain for some time unassimilated, and thus retard those changes which it is natural for this substance to assume?"

"Or, rather, shall we believe that the vital energy is preternaturally excited in active disease, and thus, by its effects on the blood-vessels, protracts the period of coagulation?"

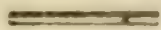
It must not be forgotten, that there is an increase in the relative quantity of the fibrine to the serum, *in the latter stages*

of most acute diseases; and, also, that the size coat appears in diabetes, when the vital energy is much depressed, and when the blood is not in a state of inordinate tenuity.

We pass over several judicious remarks relative to the application of those facts in the practice of medicine, to the consideration of the *changes produced in the quality of the serum* by disease. On this subject Mr. Thackrah adduces some interesting observations from other authors, but nothing novel of much importance. The same remark will apply to the subject of *depositions from the blood* in disease.

That there is a disposition to *putrescency* of the blood in many diseases, is satisfactorily proved, in spite of the scepticism and objections of those who *absolutely* abandon all considerations of a humoral pathology. This *putrefactive process* Mr. Thackrah believes to be influenced by the state of the system. He says, "it takes place most readily in a debilitated condition. Blood subtracted during inflammation, assumes this change later than blood during weak vascular action." This would lead us to attribute it to the same laws that regulate the coagulation of the blood.

The rapid and partial analytical sketch we have here given of the work of Mr. Thackrah, is produced, as we before observed, rather from a desire that our journal should comprise some of the valuable matter it contains, than from an attempt to convey, in an abstract, any thing like an appropriate idea of its contents.



Hints of a new Idea of the Nature of the Cellular Tissue. By G. M. DE FELICI, M. D. Public Demonstrator and Conservator of the Collection in the Museum for Pathology and Comparative Anatomy, in the University of Padua. 8vo. Padua, 1817.*

ALTHOUGH, as we have already observed, it was our intention to give a generally comprehensive view of the pathological

* *Cenni di una Nuova Idea sulla Natura del Tessuto Cellulare.* Del Dottore G. M. DE FELICI, Pubblica Répertitore e Custode de' Gabinetti Patologico e di Notomia Comparata, nella I. R. Università di Pavia. 8vo. Pavia: Bizzoni. MDCCCXVII.

doctrine at present prevalent in Italy,* before we particularly noticed treatises on distinct subjects; yet we have been induced to deviate from that intention on the present occasion, from the observations contained in this little pamphlet being almost the only ones of importance relating to the structure of the elementary tissues, that have been advanced since the time of BICHAT; and from the consideration that they may be useful to the reader in his reflections on the work of Dr. BARON. We shall present our readers with the substance of it, without adducing any particular observations of our own, excepting some references to authors who have treated on, or produced illustrations of, the same subject.

The cellular texture enters into the formation of every other part of the human body; and of many of them it constitutes the greater proportion of their structure. It was long since considered by some physiologists as a net-work of vessels, more or less pervious to the fluids; but this opinion has not been generally adopted. HALLER regarded it in a different manner: he says, "*Vasa, quæ tunicas pingunt, accessio sunt cellulosaë telæ, neque constituunt naturam membranæ, sed membranæ per cellulosaë telam innataë superadduntur.—Cellulosa fabrica fibris fit laminisque solidis omnibus, neque cavis, neque vasculosis, etsi vasculis adcedentibus pingatur.*"† A similar notion was entertained respecting it by BOHN,‡ BORDEU,§ and BICHAT,|| the principal of the original authors on this subject who have written since Haller. But Dr. Felici observes, the very accurate observations of some great anatomists seem to show the correctness of the first-mentioned opinion; and he adduces some peculiar to himself, that contribute to establish its propriety.

"RUISCH," the author says, "had already observed, that

* We are waiting for some important works that have appeared since our observations were made and arranged; and this will occasion the postponement of our Essays on this subject to the ensuing volume of this Journal.

† *Primæ Lineæ Physiol.* c. i.

‡ *De Continuationibus Membranarum.* Amstelodamiæ, 1763.

§ *Recherches sur le Tissu Muqueux, ou l'Organe Cellulaire.* Paris, 1767.

|| *Anatomie générale. Système Cellulaire.* Art. i. § 1.

the interior parenchymatous structure of the spleen was nothing else than an aggregate of vessels; and he showed some specimens of that organ, injected according to his own mode, in which nothing could be seen but an admirable tissue of vessels, ramified in a prodigious manner; as SABATIER* has also stated. HEISTER injected, by the veins of the penis, the glans and corpora cavernosa, which were thus rendered in appearance, a plexus of venous vessels.† Similar injections made by Prof. REZIA are preserved in the anatomical cabinet in the university of Padua. But the conclusion I shall hereafter advance, is also founded on some other observations made by Prof. CAV. MORESCHI.

“The corpora cavernosa of the penis, from the most careful and repeated observations and injections made in the human subjects, and various animals, especially in the horse of Prof. PANIZZA, appear to be nothing more than a mass of vessels, forming layers, anastomosing with each other, and which give to those parts the spongy, alveolar, or cellular, appearance, those bodies present. The other cavernous structures, that of the clitoris as well as that of the papillæ of the breasts, are obviously of the same nature. The cavernous tissue which surrounds the vagina, *plexus vaginam pene totam ambiens*, is merely formed by a multitude of vessels, transmitted from the adjacent parts, communicating with each other by frequent and short anastomoses. The cavities seen within the texture of the uterus communicating together, which much resemble those of the spleen and the corpora cavernosa of the penis, and which are enlarged and distended during the time of menstruation and pregnancy, are also shown to be only vessels passing into that organ in a serpentine direction. The truth of these observations is clearly demonstrated by the beautiful preparations in the Anatomical Museum in our University.”

May we not then consider, says Dr. Felici, that the cellular tissue is formed in the same manner as the spongy texture of the spleen, and the corpora cavernosa of the penis? Are not, making an abstraction with respect to size, the net-works, the

* *Traité complet d'Anatomie*, tome iii. de la Rate.

† *Compendium Anat.* tab. v. fig. 22, 25.

cellules, and the alveoli, the same in one as in the other? And why should we then regard their formation in a different manner? And are there not many circumstances which show the correctness of the opinion above advanced?

“If we observe the back of the hand,” the author continues, “we see a tissue constituted solely of an extremely fine vascular net-work. All the arteries which are distributed in the membranes have also the same reticular figure. ‘*Fines arteriarum,*’ says Haller, ‘*cylindrici, aut cylindricis proximi, ramulos producunt in eadem longitudine crebriores, plerumque in rete dispositos, ut quilibet ramus minoribus surculis cum utroque vicinorum anastomoses faciat. Ita in omnibus membranis reperitur.*’ And, speaking of the veins, he adds, ‘*Rami majores venarum magis reticulati sunt, ac frequentioribus anastomosibus, nec inter parva, sed inter maxima vasa, inter vicinas, dextras et sinistras, superiores et inferiores venas, ubique adparentibus coeunt.—In minoribus ramis, retibusque membranis, viscerum, que interiori fabrica arteriarum, venarumque plerumque conjunctæ ambulant. Minus fere flexuosæ sunt.*’* ”

Many beautiful and fine injected preparations in the Museum at the University of Padua, some visible to the naked eye, others only by the aid of the microscope, as well as others furnished by Professor SCARPA, show in the most evident manner the cellular tissue of the face, the palms of the hands, the soles of the feet, the villi of the intestines, &c. to be nothing else than a congeries of vessels. “How surprising are the injections with mercury of the lacteals of the small intestines of the turtle, in the specimen contained in our Museum. In this preparation, presented by Prof. Scarpa, the lacteal vessels are so numerous, that it might be said the intestines of this animal are merely a congeries of those vessels; the metal shining through every discernible point of their surface. These injections would frequently succeed if the necessity of injecting the lymphatics by the branches, leaving the trunks to avoid the opposition of the valves, did not present a forcible obstacle.

“The injections of Ruisch prove the same thing; and yet these injections, however happily they may have succeeded, never fill the whole of the most minute vessels.”

* *Primæ Lineæ Physiologiæ*, c. ii. §§ 38, 48.

The beautiful observations of Prof. Scarpa, too, on the nature and generation of the bones, prove that they are not formed by lamellæ, or fibres, as formerly believed, but by a reticulated, tomentous, cellular tissue:* these, with the injections of the bones in general made by the same physiologist,† seem to show that the basis of the osseous structure is nothing but an aggregate of vessels.

These notions respecting the formation of the cellular tissue are powerfully supported by the phenomena attending the formation of new parts in the healing of wounds, the union of fractures of the bones, the generation of false membranes and cysts, which are apparently wholly formed by fine vessels shooting into the effused lymph. These are circumstances that at least show the subject to be worthy of attention, in the manner the author has here pointed out. Dr. Felici considers that it is from an alteration in the state of these vessels, that thin, diaphanous, and pellucid, membranes become dense and opaque in inflammation. "Thus," he says, "the fine and transparent lamina of the conjunctiva, by dilatation, varicosity, elongation, and obstruction, of the veins, becomes thick and opaque, pulpy, flocculent, and of a reddish hue; so that, on a first view, it would appear that a new membrane was generated on the cornea."

Whenever the cornea is affected with cloudy specks, a congeries of varicose venous vessels will be found on the white part of the eye, corresponding to those specks, more elevated and convoluted than the other sanguineous vessels of the same order; and, if the cornea be clouded in more than one point of its circumference, there will be seen so many distinct plexuses of varicose venous vessels distributed on the eye, exactly corresponding with them. It appears, on a first view, that each of those venous congeries, so distinct from, and prominent above, all the rest, have forced a passage for the blood through the boundaries of the sclerotica to the cornea. "I have an eye preserved," says Prof. Scarpa, "taken from the body of a man affected with chronic varicose ophthalmia, accompanied with cloudiness of the cornea, who died of inflammation in

* *De Penitiori Ossium Structura Commentarius*. 4to. Lipsiæ, 1799.

† See *Index Rerum Musei Anatomici Ticinienis Antonii Scarpa*.

the chest. Having injected the head by the arteries and by the veins, I found that the wax, with which the veins of the conjunctiva were well filled, had found a free passage, as well in the more elevated congeries of those veins, as in the more minute branches of the same congeries running in a serpentine direction on the surface of the cornea, in the precise spot where the cloudiness had existed; whilst, in all the rest of the circumference of the cornea, the injected wax was arrested by having found, in the confines of the cornea and the sclerotica, an insuperable obstacle. And it is wonderful to see in this eye, by the aid of the lens, the extremely fine net-work formed by the numerous venous branches in the limits between the cornea and the sclerotica, where they beautifully anastomose together in a thousand ways all around it, without any of them, excepting in the spot corresponding to the situation of the opacity of the cornea, passing the boundaries formed by strong adhesion, which the fine lamina of the conjunctiva there receives. The *pterygium* is originally produced by an increased dilatation of minute venous vessels passing over a particular part of the surface of the cornea; to this is then added a greater thickness than natural, and an opacity of the fine lamina of the conjunctiva which covers the cornea, on which those vessels are spread. The facility and promptness of the cure in these cases, by excision of the congeries of vessels on the conjunctiva, and a concentric incision at the margin of the cornea, by which this regains its original pellucidness in the short space of twenty hours, is a proof that this disease is kept up more by a varicose and elongated state of those vessels, still pervious, than by an effusion of serum or coagulable lymph in the delicate lamina of the conjunctiva which is spread over the external surface of the cornea.”*

On this principle,—that is, from the generation of new vessels, or from the elongation of the extremities of those of the inflamed part,—the author observes, it is, that the generation of new membranes in inflammation depends; and he refers to preparations in the Museum of the University of Padua, in which membranes of that kind, formed in peripneumonia,

* See the new edition of the *Saggio di Osservazioni e di Esperienze sulle Principali Malattie degli Occhi*, di ANTONIO SCARPA; capi 7, 8, 11.

carditis, pericarditis, and abdominal inflammation, were filled with mercury by injection. Preparations illustrating this vascular union in the morbid adhesions occurring in inflammation, have been presented to it by Scarpa, Borda, and Joseph Frank. The following case contributes to the support of these opinions.

On dissecting a body, accompanied by Prof. Panizza, Dr. Felici says that he found a large, isolated, cystoid tumour in the left hemisphere of the brain, which contained about eight ounces of coagulable lymph mingled with pus of yellowish-green colour. The lateral ventricle, which was adjacent to it, was driven considerably out of its natural situation, in a downward and backward direction. The cyst which contained the fluid above mentioned, could not, apparently, have been formed by the membranes of the brain. On examining it particularly, Prof. Panizza found it to be composed of two laminæ, extremely vascular: the external one adhered to the cerebral mass by ramifications of vessels; the other presented a granulated appearance on its internal surface, was of a red colour, and looked indeed as if it had been injected.

“These facts, the author continues, will explain the nature of the tubercles found in the skin, which are merely a network of vessels, as is demonstrated by injections, with the exception of some nervous fibrili; which, though we cannot trace them, we know to exist from their effects.*

“We may thus explain the generation of new parts in wounds, by the elongation of the divided extremities of vessels into granulations, which restore the soft parts to their original form: the same means also produce the union of bones.

“We may thus explain the obstinacy of ophthalmia, the opacity and condensation of the conjunctiva in chronic varicose ophthalmia, in cloudy specks on the cornea, and in pterygium, by the excessive distension suffered by the vessels of the conjunctiva during the inflammatory state; by the varicosity and elongation of the vessels, principally the venous; and, consequently, by the condensation of the conjunctiva produced by the inflammatory action.

“We may thus explain the generation of new membranes,

* CALDANI, *Institut. Physiol.* c. 16, §§ 224, 225, 227. FATTORI, *Galda allo Studio dell' Anatomia Umana*, §§ 367, 368.

the morbid adhesions between them and other parts, and the origin of cystoid tumours within the substance of the brain.

“ We may thus also account for the *vasa vasorum*, which enter into the formation of the cellular tissue of the vessels themselves, of which HALLER observed, ‘ *Arterias arteriæ habent in extremo imprimis celluloso textu, a vicinis undique arteriosis trunculis natas, multas, ramosas, reticulatas, omnes exiguas, in fetu etiam absque injectione plurimas.*’* ”

“ The same fact will enable us also to comprehend more satisfactorily the sudden redness that tinges the face in the act of blushing, as well as the paleness of the skin and the cutis anserina from terror, or from cold; by the influence of the nervous action on the contractile power of the arteries, and the varieties in the transmission of the blood by them which thence arise.

“ We may thus explain the growth, the preservation, and the death, of animals, according as these vessels are elongated, and give way to the impulse of the blood, or by degrees successively become closed and obliterated; and, lastly, according as their excitability shall become destroyed by the influence of stimulants.

“ Lastly, we may thus more easily explain the secretions over the whole external and internal surfaces, and in the cavities of the body, and the re-absorption; and the readiness with which adipose matter is deposited, as Haller observed, ‘ *Via ab arteriis ita proxima et libera est in cellulas adiposas, ut majuscula ostia eo hiare necesse sit, quæ dimittant mercurium, aerem, aquam, gelatinam, oleum, semper etiam in vivo animale pigrum. Non per longos aliquos propriæ fabricæ ductus excernitur, sed per totam arteriæ longitudinem undique exsudat, ut nulla pars circumpositæ cellulosæ telæ absque madore sit, quando arteria aqua repletur. Adeps calidus in arterias pulsus easdem vias facillime legit. Celeriter congeritur, argumento cito renascentis post acutos morbos obesitatis.*’† ”

“ Considering, then,” the author concludes, “ the whole of this extensive tissue, not as a texture of no importance, except as a basis for sustaining the figure and firmness of the

* *Prim. Lin. Phys. c. ii. § 32.*

† *Prim. Lancæ Phys. c. i. § 19.*

body, but as an aggregate of vessels, the immediate organ of the secretions, of nutrition, of absorption, &c.; and viewing it entering into the formation of, and furnishing life to, every part of the system; we shall form a more noble, and probably more correct, idea of its uses in the animal economy."

These notions, Dr. Felici is careful to observe, are not advanced in a confident manner, but with the intention of exciting the attention of anatomists to some new researches on the subject. The observations he has adduced, seems to be sufficiently precise and important to lead to such an investigation; and the views they disclose of the structure of the body generally, certainly must facilitate our reasonings on what we witness of its functions.

Reflections on the Nature and Treatment of Cancer, supported by Practical Observations. By J. J. LASSERRE, of Domme, Doctor in Medicine of the Faculty of Paris.*

[From the London Medical and Physical Journal, for November, 1819.]

THIS memoir contains some observations of remarkable interest to every medical practitioner, whatever general principles of pathology he may have adopted. Even those who consider that *cancer* is, in all cases, essentially a disease of a specific nature, or local morbid action modified by a peculiar diathesis of the system, will not think the cases here related unworthy of their serious attention. We shall therefore select two of these which are the most interesting, and such of the remarks of Dr. Lasserre as are expressly applicable to them; omitting his more general theoretical reflections, because we have already given our readers some ideas of them, on various occasions, in our extracts from the work of M. BROUSSAIS, of whose principles the author is a zealous partizan.

"A female peasant, of a dark complexion, robust habit, forty-seven years of age, married since her twenty-third year,

* *Reflexions sur la Nature et le Traitement du Cancer, avec des Observations à l'appui.* Par J. J. LASSERRE, D. M. P. à Domme (Dordogne.) *Journal Universel des Sciences Médicales*, tome xiv.

and who had never been a mother, experienced, during eight months, great irregularities in the periods of the occurrence of the menstrual evacuations, which were always accompanied, too, by violent colics and severe pains about the loins. In the course of the month of May, 1817, she perceived a moveable tumour seated above the right nipple, about the size of a large plum. For twelve days she had suffered lancinating pains in the part, which induced her to come to consult me. This was in the ensuing June. The tumour was not painful to the touch; the nipple was redder and more sensible than that of the opposite side. I enquired whether she had ever experienced any external injury to the part, which might have given rise to it, and if she had adopted the bad habit of confining her person tightly by her dress. To these questions she replied in the negative.

“Reduced in this case to regard the affection as consequent on the derangement of the functions of the uterus, and as independant of local mechanic action, I proposed the extirpation of the tumour by the knife.*

“Forced to combat an affection which menaced a rapid progress from the violence which the pains had acquired for several days past, and the rapid increase of the size of the tumour, I advised the repeated application of leeches to the part itself, and, intermediately, the plaster *de Vigo cum mercurio*, mingled with that of *cicuta*;† an issue in the right arm; half a drachm of mercurial ointment to be rubbed on the same member; bleeding from the arm every eighth day; the use, after intervals of four days, of thirty grains of the pills of Bellost; a severely restricted diet, carried so far as to excite a sensation of *sharp* hunger; and cessation from the use of wine.

“On the 20th of July, thirty-six days from the commencement of the treatment, this woman again visited me. The tumour had diminished to about the size of a small nut; the pains were so slight as to be scarcely sensible; and the nipple was

* Should we, in symptomatic cancer, have recourse to the operation in the first instance?—*Note of the Editor of the Journ. Univers. des Sciences Med.*

† “In employing this topic, I had no other object than that of alleviating the pain by means of the sympathy of the skin with the parts which it immediately covers.”

neither redder nor at all more sensible than that of the other side. Menstruation had been totally wanting. The treatment having succeeded beyond my expectations, I ordered its continuance, and added to it the use of whey containing the acetate of potash. Leeches were employed only twice in the ensuing month, as well as the bleedings from the arm.

“The 20th of August. no appearance of menstruation; there did not remain the slightest vestige of the tumour; the pains had ceased for three weeks. The only thing this woman now experienced from her disease, was the emaciation resulting from the forced abstinence, to which she had submitted with a degree of resignation truly exemplary. I have since then frequently seen her: she has regained her health and flesh, and her ordinary gaiety of disposition; not the slightest symptom of her disease has re-appeared. The breast in which the tumour was seated is, however, not so firm and plump as that of the other side. This woman still preserves the issue open, and wears the plaster on the breast, which she renews every month.

“I should by no means say that I have, on this occasion, cured a cancer of the breast; I am too well convinced that the disease was not a cancerous affection: but I believe I may state that it is highly probable that I have prevented the development of a disease of this nature. Almost all cancers have not a different origin; they are developed under the influence of identical circumstances. If it be demanded of what nature was this woman's disease, I reply, that it was chronic inflammation depending on the influence of sympathy from the uterus; or, rather, from the cessation of the functions of that organ, and the transport to the breast of the sensibility which then abandoned it, perhaps too suddenly.—

—“What I have said respecting chronic engorgement of the glands of the breast, is applicable to disorganizative affections of the neck and body of the uterus. Let us reflect on the circumstances which evidently give rise to cancer of the uterus, and examine them in a physiological point of view, and then see if that malady is not obviously the consequence of prolonged irritation? I need only mention difficult labours, and a want of proper relation between the genital parts of the two sexes in the act of coition. Every physician knows that cancer of

the uterus may occur as a consequence of these circumstances.

“This etiology is equally applicable to cancers of the stomach, the rectum, &c. In the greater number of these cases, we only find, if we trace the source of the evil, the slow progress of chronic inflammation, which leads to them as a result. Let it not be said that cancer is a malady *sui generis*, an *essential* malady, an *organic vice*. If all diseases had been regarded in this manner, medicine would have been always left in its infancy. The cultivation of pathological anatomy, the application of the laws of an enlightened physiology, the study of the language of the suffering organs, independently of the names by which diseases have been designated: such are the only means of making discoveries in medicine really useful to the welfare of patients and to the progress of science. It is to M. Broussais that we owe the first clear exposition of these principles, and the demonstration of the superiority of them by their application to the healing art.

“One case only of chronic engorgement, or, to speak the ordinary language, one case only of scirrhus of the uterus, has occurred to me in the course of my practice. Without saying any thing in prejudice of the propriety of the means I employed to combat it, I will here faithfully relate them.

“Mrs. T. forty-three years of age, of a dark complexion, prodigiously fat, and of an ardent disposition of mind, experienced for six months irregularities in menstruation. This lady was the mother of five children: the whole of her labours had been of the most easy and favourable description.

“In the course of the summer of 1817, Mrs. T. began to experience dull pains and a sensation of heat in the interior of the vagina. She complained of it to her husband, who made me acquainted with it, and who much aided my exertions to obtain a full account of the case, by the manner in which he urged her to disclose all the circumstances respecting it. She at length, with extreme difficulty, acknowledged that, for five months past, the approaches of her husband had been the cause of very severe pain. She experienced a sensation of weight in the hypochondres, thrilling pains about the loins and the groins,

and occasionally painful dartings in the lower part of the abdominal cavity, and in the interior of the genital organs. As she was alarmed at these circumstances, from the recent death of one of her friends, who, after symptoms which she said were similar, had been afflicted with cancer of the uterus, Mrs. T. permitted me to make an examination of the seat of the disease.

“ I found the neck of the uterus hard, unequal, and painful on the slightest pressure: the body of this viscus appeared to be larger than ordinarily, although pressure immediately on it, or above the pubes, did not shew it to be possessed of unusual sensibility. The finger caused a sensation of burning pain in the vagina. The patient had never had any other flux from these parts than the menses and the lochiæ.

“ I directed that six leeches should be applied to the vulva, and two days afterwards the same number to the groins, and so on alternately, until they were counter-ordered; a hip-bath daily, made with a strong decoction of poppy-heads; an enema morning and evening, of a slight decoction of the fresh leaves of belladonna; an issue in the left arm; and the most severe restriction in diet, with total abstinence from wine.

“ After this treatment had been followed for fifteen days, the pains were much diminished. The patient endured the restriction from food with the greatest impatience; but, on my insisting on the necessity of this measure, she consented to submit to it. Fomentations with the decoction of belladonna were made every evening to the hypogastrium, in addition to the other measures.

“ On the 16th of July, the patient had become considerably emaciated; the pains were much less intense. The enemata and fomentation to be discontinued. The leeches were applied only once during the rest of this month. The same severity of regimen. The food consisted solely of vegetables,—as carrots, the fruits of the season, and about four ounces of bread daily. Her drink was merely water and lemonade.

“ The 12th of August, the patient suffers no pain, either in the loins, the abdominal cavity, or the groins. The neck of the uterus has regained almost its ordinary size; it is soft,

equal, and not painful on pressure; the sensation of burning heat is no longer experienced in the vagina. Menstruation has not appeared since a month before the treatment. The hip-bath to be continued; four grains of extract of cicuta to be taken daily, the abstinence to be less severe.

“ The 8th of September, the little tumefaction of the neck of the uterus last noticed has totally disappeared. The treatment was here omitted, and the patient gradually resumed her ordinary regimen. I thought it my duty to represent to her husband the necessity of not again using cohabitation. Since this time no accident has appeared; and I think I may assert that the affected is cured.—

—“ In all the cases I have related, it has been seen that I have insisted on abstinence from food to such an extent as to produce *sharp* hunger. The want of alimentation is one of the most favourable conditions for the cure of affections in which it is desirable to effect the re-entry into the circulation of the product of morbid accumulation of nutritive matter, in consequence of inflammation either acute or chronic.”

The author concludes with observing, that he has found mercury very efficacious in fulfilling the latter intentions, when employed in the mode, and with the precautions, pointed out by physiology.

ORIGINAL PAPERS.

 FOR THE ECLECTIC REPERTORY.

Case of Pneumonia Typhodes, with remarks. By JAMES NORCOM, M. D. of Edenton, North Carolina, in a letter to DAVID FREEMAN, M. D. New York—Communicated by DAVID HOSACK, M. D. of New York.

ON the 12th of January last, I was called on to visit Mr. Gabriel L. Stuart, residing in Martin county, on the river Roane Oak, about eight miles above Plymouth, and twenty-eight miles from this place. Upon enquiry, I learnt that he had been taken about five days before with a chill, which was succeeded by fever, pain in the side and breast, dyspnœa, a considerable expectoration of bloody mucus, great heat and thirst, and an overwhelming sense of debility and oppression. Before I saw him, Dr. Picot, a physician of Plymouth, had attended him; had bled him twice; given him an emetic, and two or three purges; applied two small blisters on his chest; and kept him pretty generally under the action of *common febrifuge* medicines. But he had not been relieved by any of these remedies: on the contrary, his debility had increased, and every symptom of his disease became aggravated. At the time I saw him, the following were the symptoms that manifested themselves in the case: a pulse beating about 132 strokes in a minute, quick and active, but without fulness, hardness, or tension; a confined, anxious, and hurried breathing; frequent fits of laborious coughing; a copious bloody expectoration: pain under the sternum extending to the left side; some thirst; a hot, but not a dry skin; irregular flushings of the face, with great restlessness and debility. His bowels were regular, and his urinary secretion natural, in quantity and quality. His

appetite was weak and fastidious. He was rather drowsy, and seemed a little agitated and incoherent, at times, though not delirious. In the intervals of his slumbers, from which he was easily roused, he had a singultus; yet he had scarcely any subsultus, or what is usually called catching of the nerves. After setting a short time in his room by the side of his bed, with my fingers on his pulse, and my eyes steadily fixed on his countenance, I discovered the flushing of his face to be sometimes on one side and sometimes on the other; and, now and then, a circumscribed red spot on one or both cheeks inclining to a livid colour.

In this state of things, reflecting that all the circumstances of the case had been growing worse under the remedies he had been using, and entertaining a very different view of it, from that upon which they had been prescribed, I immediately laid aside the febrifuges and evacuants, that had been directed for him, and applied a large blister over the sternum, gave him a dose of Dover's powder, and directed a cup of wine whey, with two or three ounces of the infusion of serpentaria, every two hours during the night. On the morning of the 13th, finding his pulse not increased in force, and still beating 120 strokes in a minute; his pain, cough, expectoration, and breathing, the same, or, if any thing altered, rather worse; and his restlessness and anxiety still distressing, I gave him a drachm of bark every two hours, in a glass of wine, interposing wine whey and sangaree as common drinks; and directed his room, which hitherto had been kept very cool,* to be warmed with a com-

* There certainly cannot be a greater error committed in practice, in typhus and typhoid diseases, than that of placing patients in cold or very cool rooms; more especially in the season of winter, when such diseases most frequently occur. I have uniformly recommended, in all such cases, that temperature which was most agreeable to the sick; and have found it, not only comfortable, but extremely beneficial. Even a temperature, which is perfectly pleasant to persons in health, will often require to be raised, to prevent our patients from being injured by the operation of cold—a principle the most hostile to the health of the human body, and the most difficult to be counteracted in its influence upon human diseases. If it were not essential to the *comfort* of our patients, in cold weather, to warm their chambers with a fire, it is undoubtedly necessary for the purpose of *change* and *ventilation*: For, nothing so certainly conduces to a change or renovation of

fortable fire. In a few hours after beginning with the bark and wine, his cough and expectoration nearly ceased, and his breathing, instead of becoming more difficult, contracted, or laborious, as my colleague apprehended would happen, seemed evidently to be better; his pulse had gained magnitude or volume, but still continued frequent and destitute of strength or hardness. In the evening I desired a glass of wine should be interposed between the doses of the bark, still using the whey and sangaree as auxiliaries, and administering nourishment at convenient times. About ten o'clock I gave him twelve grains of Dover's powder, with a tea-spoonful of paregoric elixir. He rested well during the night, had little or no thirst, and slept sufficiently; he had a pleasant breathing perspiration, and bore his medicines well; and on the morning of the 14th I left him apparently better, *in every respect*, directing that the bark, wine, and nourishment should be continued day and night, without intermission, and that opium and camphor, a grain of each, should be given him at night, and blisters be applied to his extremities, in the event of delirium supervening, or his pulse becoming more frequent or languid; and recommended, further, that French brandy toddy should be substituted for the whey and sangaree, as his ordinary drink. From this time, I was absent from him till the evening of the 15th, about six o'clock, when I saw him again, and found him with a pulse beating from 100 to 108 strokes in a minute, quick, and somewhat full, having gained considerably in volume, but still soft and compressible, and totally devoid of hardness or tension; his cough much abated; his expectoration moderate and less bloody; his pain greatly diminished; his skin moist and open,

the air in a sick room, or so certainly promotes atmospherical purity and circulation, as a good fire. It is moreover so palpably inconsistent, in the cases of persons labouring under diseases of weak or deficient vascular action, to expose them to the agency of a strong sedative, *for I must still use this unfashionable term*, that I am astonished the practice has not been more generally deserted. Heat is, indeed, the source of all circulation; and perhaps, of all action and vitality in animal bodies. How unwise, then, and unphilosophical must it appear to exclude it from the apartments of the languid and feeble victims of typhus, to whom it often affords pleasure, and always promises safety and relief.

and his breathing rather more composed, but still quick and contracted. He was directed to continue his medicines; and, as he had not had a discharge from his bowels since the morning of the 13th, and lest the large quantity of bark and nourishment accumulating in his intestines should oppress him, or lessen his ability to pursue the course prescribed, I advised a mild aperient injection to discharge their contents. It gave him a considerable evacuation, but was succeeded by a state of alarming debility;* and the fatigue of getting out of bed, which he could not be prevented from doing, nearly overcame him—so that his pulse for several hours afterwards beat from 120 to 126 strokes in a minute; his respiration became as hurried and oppressed as ever; his expectoration became more coloured; he grew considerably delirious; and had more subsultus than had occurred during his confinement, with a sensible increase of that very formidable symptom denominated singultus, or spasmodic sighing. I now gave him an anodyne, composed of a drachm of paregoric elixir and thirty drops of laudanum, applied blisters to his legs, and exhibited the bark, wine, and toddy, with renewed assiduity. By the morning of the 16th I succeeded in getting him back to the point at which he stood, before the exhibition of the enema, his pulse being reduced to 108 strokes, his breathing having become more quiet, and his strength having considerably improved. The same course of treatment was pursued until the morning of the 17th, when his pulse beat 102 strokes in a minute, his breathing being more slow and tranquil, his expectoration more free and easy, and less coloured; his delirium, pain, subsultus, and every other unfavourable appearance, having entirely disappeared. The paregoric and laudanum were repeated every evening from the time of its being first exhibited, and always enabled him to rest well. On the evening of the 16th, and twice on

* From the effect of a mild injection only, on this patient, it will appear satisfactorily, to every reflecting physician, that he could not have borne any kind of depletion; and I cannot help thinking it highly probable, that the smallest deviation from the plan pursued, or only a few hours suspension of the stimulants by which he was supported, at any time, between the sixth and ninth days of his disease, would have placed him out of the reach of all human skill and assistance.

each of the two succeeding days, he had a spontaneous discharge from his bowels, which made it necessary to combine a little laudanum occasionally with the bark. Pursuing this mode of management, I left him on the morning of the 19th, being the eleventh day of his disease, in a promising condition; having gained considerably in muscular strength; being free from subsultus and delirium; his cough unattended with pain; his skin soft and moist; his bowels regular, his appetite increasing; and his pulse* beating 90 strokes in a minute.

From the period last mentioned, Mr. Stuart continued to mend, and has for several days past been diminishing the quantity of bark and brandy prescribed for him, being able to take plentifully of nourishment, and to drink wine with satisfaction; indeed, I consider his case happily decided. Presuming, therefore, on his recovery, and regarding the practice in his disease, and its effects upon the predominant symptoms of it, as highly exemplary and remarkable, I think I cannot render you a more essential service, than by impressing it on your recollection as the most important, interesting, and instructive one, that can possibly occur to the observation of a practitioner of medicine: inasmuch as it proves, most incontestably, that all the symptoms, or, at any rate, a very large majority of them, which usually characterise a sthenic, or inflammatory disease, may occur in a state of asthenia, or weak mor-

* It will be observed, that in the history of this case, I have paid particular attention to the state of the pulse; and I wish it to be remembered that, in my description of it, I have not trusted in the least degree to memory; having, in every instance, noted the impression it made on my mind, while my fingers were on the artery, or in a very few minutes afterwards. An attention thus accurate and particular, in observing the action of the pulse, is all important in the management of typhoid diseases. The appearance of activity, so often remarked in such cases, is very apt to mislead the inexperienced practitioner; but, upon due examination, the want of strength in the pulsation of the artery will generally set him right. It is certain that no dependence can be placed on the appearances of the blood, which has almost always been found buffy, and often extremely dense and tenacious throughout the disease. In the case before us, it was pronounced to be very inflammatory; and every one must perceive, upon the most superficial survey of the complaint, that there was no indication, except that derived from the state of the pulse, which might not as well have led to the use of the lancet and other evacnants, as to the practice that was ultimately adopted.

bid action of the vascular system. For here we see heat, thirst, redness, pain, cough, and dyspnœa, the ordinary and supposed essential concomitants of inflammatory excitement, yielding to the impression of remedies which, under the influence of common pathological views, would seem most eminently calculated to increase them. How, then, shall we speak of such a disease, without banishing from our description the term inflammation altogether; or without giving up all our settled ideas respecting the nature, phenomena, and essence of inflammation? Surely that cannot be inflammation, in any ordinary or correct acceptation of the word, that may be relieved and cured by those agents which have been known invariably to increase its most acknowledged characteristics. If not inflammation, by what name shall it be designated to enable the physician to recognize and comprehend it? I have long been of opinion, from having seen and treated many cases similar to the one above detailed, that every morbid phenomenon produced by acute or active inflammation, excepting only some of the morbid states of the pulse, may be occasioned by a state of action or excitement diametrically opposite to that on which it has been generally supposed to depend; and that preternatural heat, pain, redness, tumour, thirst, itching, and burning, or smarting, are, *in many instances*, as certainly and truly the effects or attendants of deficient action, excitement, or circulation, in an asthenic or typhous state of the system, as they are of excessive force or excitement, in the sthenic or inflammatory state. In my studies and reflections on this subject, I have always felt inclined to object to the term inflammation, except in cases of external and local disease; and even here, I think it will be found necessary, upon a more diligent and careful examination of circumstances, to use it with greater regard to its common import, or to affix some new definition to it, in some of its less frequent forms and appearances.—For I am perfectly satisfied, that external as well as internal inflammations may be, and frequently are, so differently constituted or characterised, as to be extremely different, at different times—nay, directly the reverse of each other; and that, too, when

in all their sensible and more obvious signs, they very nearly resemble each other.

If the foregoing conclusions be correct, and inflammation in pneumonia may be typhus or synocha, that is, attended with feeble, morbid action, or with symptoms of elevated excitement, why may not the same thing take place in cynanche, in phrenitis, in hepatitis, in enteritis, and in every other case of fever with local inflammation, or that can be determined by local predisposition to any part of the human body? It unquestionably does, and I have no hesitation in saying that it is for want of a knowledge of this distinction, that our winter practice is so frequently unsuccessful. The distinction has long been observed and admitted in erysipelas and some other forms of external inflammation, but the principle on which it depends, seems not yet to have been clearly understood, or applied extensively enough, to diseases of a more general nature, affecting internal parts. Would you have proof that such symptoms as I have described, may proceed from debility or want of action? Lay your hand on the crimson cheek of a patient considerably advanced in typhus; and, as often as not, you will perceive it colder than the surrounding surface. If this does not satisfy you, apply your palm to the apparently glowing surface of a limb affected with erythematic inflammation, and you will often be sensible of a coldness below that of the medium in which it lies. But perhaps you are not yet convinced—then carry the cool draught from the crystal stream to a patient burning up with thirst in the last stage of cholera, where all action and circulation are gone, and ask the miserable sufferer, if his thirst is allayed, or his sense of inward burning relieved? What will be his answer? “Water—water, cold water:”—“Oh! I would give my estate for one gallon of water.”

It would be easy to multiply observations and reflections on the particular symptoms of the case I have described; but they will readily suggest themselves to every attentive enquirer, and cannot be made more impressive or striking than they appear, in the simple and undisguised narrative in which they are given.

Edenton, North Carolina, February 1, 1820.

On the Effects of Prussic Acid in Pulmonary Consumption.—

Extracted from a letter of R. BASSETT, Esq. to H. Ather-ton, Esq. of Philadelphia, dated Westmoreland, New York, February 18, 1820.

You ask me what effect the prussic acid has had?

Believing that it may be in your power to communicate it to some one labouring under phthisis pulmonalis, and thereby benefit suffering humanity, I will be as particular in answering your question as the limits of a letter will permit. The first account which I saw of the use of the acid was contained in a letter from professor Smith, of South Carolina college, to Dr. Henderson, of Raleigh, North Carolina. I will presume that this letter was published in Philadelphia, and has been seen by you. In my last, I informed you of what had been the state of my health for the greater part of the year past, more particularly during the summer. My case, I believe, was considered hopeless, not only by my friends, but also by the physicians who had occasionally seen me. When I saw Dr. Smith's letter, I informed my attending physician that I was desirous of trying the acid, that my condition was such then, that I was willing to be the subject of fair experiment. Professor Noges, of Hamilton college, undertook to make the acid, and I commenced taking it about the first of September. I began, according to the direction of professor Smith, with three drops per twenty-four hours, in as many ounces of cold water; and used in small quantities once, twice, or thrice in an hour, indifferently. I increased the quantity gradually, from three to ten drops, taken in twenty-four hours. I can now take ten or twelve drops in the course of ten hours, without any serious inconvenience. After having taken it two or three days, I began to feel the effects mentioned by professor Smith, as a sense of stricture across the breast, which was for several days quite painful, especially at night. The brain is sometimes also affected. The patient is relieved from these effects by suspending the use of it a few days, or by the discharge of some blood from the lungs. Would not blood-letting have the

same effect? After I had used the acid about ten days, I was taken with a painful diarrhœa, which continued for several days. This was considered by my attending physician, and others, as the last symptom of dissolving nature. From this time I began gradually to convalesce. The cough began to abate, and in the course of a few weeks almost entirely left me. The expectoration was less and easy; the wasting hectic subsided, and I could sleep quietly the greater part of the night; which I had not done for many months before, I may say for years. I have not lost six nights sleep since the first of October. I had not six nights quiet rest for the four preceding months. My appetite also became very good, so that I can relish any food, which I had not done for the last six months. To eat had been a most painful task. The spasms, pain, and swelling of the legs almost left me, and the hurried and painful respiration, which I suffered on any attempt to walk, became much less so. When I began to take the medicine I could ride but a short distance, and not then without one to help me in and out of the carriage, and to drive. In four weeks from this I could drive myself seven or eight miles and back, the same day, without inconvenience. Could I have avoided taking cold, I have every reason to believe that my convalescence would have been uninterrupted. When the cough has been brought on by cold, I have found the acid by far the most effectual remedy. Of its effects on others, I can say but little. I am the only one in this part of the country who has made a faithful trial of it. Dr. Hastings informed me a few days since, that he was now prescribing it in one or two other cases, with the most flattering and happy effects. The acid is probably one of the most virulent poisons in nature. It should, therefore, be used with great caution. Of this quality I know but little beyond my own actual experiment. I first tried it on a cat, by giving only two drops. The effect was instantaneous. It fell on one side immediately, being totally unable to move. I increased the dose; when, after partly rising and falling several times, like an intoxicated person, it got on its feet and walked off perfectly well. Not satisfied with this experiment, I tried it on another cat. I gave four

drops on a piece of meat. The moment it was swallowed, the cat instantly fell and died, in the course of about two minutes. From the appearance of the eyes in both cases, I should suppose that the brain was very much, if not principally affected. *Quere*—Is it not a powerful intoxicating stimulant? Some of the medical students, hearing of my experiments, exhibited a few drops to a large dog, which proved instantly fatal. It is said to be equally fatal when applied externally. Of this I know nothing certain. The instances mentioned are sufficient to shew that it would be a dangerous substance in the hands of careless patients. I have made use of two other remedies, which I will briefly mention. The *Lichen Islandicus*, or Iceland Liverwort, as recommended by Dr. Reece, of London, in his work on domestic medicine, vol. 1. p. 96: the precise title of this book I do not recollect. I think I have derived much benefit from the use of the lichen. I have also used the tartarized antimony in the form of an ointment, applied to the breast, which causes small eruptions, and is an easy substitute for blistering.

FOR THE ECLECTIC REPERTORY.

On the Medical and Physiological properties of Prussic Acid.

BY ISAAC DAVIS, M. D.

THE poisonous character of prussic acid has long since been acknowledged, and the numerous cases of immediate death by subtle poisoning, may, with great justice, be attributed to the employment of prussic acid, or some of its compounds. This opinion is fully corroborated by the experiments of Coullon, Orfila, Brugnatelli, and Magendie. These celebrated men have shewn, that when prussic acid is administered to hot blooded animals, in its concentrated state, it destroys the irritability and contractility of their voluntary muscles. The few experiments which I have had an opportunity of making with the concentrated acid, fully confirm those of Dr. Magendie.

In imitation of Magendie's experiments, a few drops of the concentrated acid, prepared according to Gay Lussac's process, by Dr. Cullen of this city, were introduced into the posterior fauces of a good sized dog; the effect was more astonishing and instantaneous than had been even anticipated from Dr. Magendie's description of its powers: the animal fell on the floor and expired without a single convulsion. A few drops of the acid were thrown upon the eye of a cat, and the effect was as sudden and complete as in the former experiment. A rabbit died almost immediately from the puncture of a lancet imbued with the acid. I intended to multiply these experiments, and, at the suggestion of Dr. Cullen, I proposed to ascertain, by direct experiment, if the animal was susceptible of galvanic excitement, or not. I, however, improvidently deferred my experiments a day or so, and when I wished to resume my operations, I found the acid, which had at this time been made above three days, was entirely decomposed. Possessing very little skill in the manipulations of chemistry, I again applied to Dr. Cullen for an additional quantity of the acid; but I could not prevail on him to undertake the process a second time, in consequence of its great trouble and danger.

Presuming on the repetition of these experiments, I unfortunately overlooked some of the leading physiological phenomena produced by the acid. I did not, with any certainty, determine whether, according to Magendie, the animal retained life for any period at the expense of its external functions. Dr. Magendie, in studying the phenomena of poisoning by prussic acid, observed, that animals on which it had been made to act in more moderate doses, and in whom no traces of sensibility or muscular contraction could be found, would often continue to breathe freely for several hours, while the circulation remained scarcely altered. Indeed, says Magendie, it might be said that these animals were dead with regard to their external functions, yet still enjoyed life through their nutritive faculties.

This power of allaying general sensibility, without any apparent injury to the respiration and circulation, induced the be-

lief that this acid might be employed in diseases of great irritation and excessive sensibility.

These opinions, deduced from observing the effects of the acid on animals, soon led to its use in spasmodic affections, where local irritation would excite by sympathy constitutional affections.

As might be expected, it has been found particularly useful in spasmodic coughs, asthma, and in those of phthisis, where the patient is only debilitated by repeated bleeding and the use of other depletives; but before we enter into a detail of the many diseases in which it is found highly serviceable, we should first enquire, under what class of remedies it is to be ranked, and we will attempt to assign some more specific character than we have as yet bestowed upon it.

That it is a narcotic, no one will hesitate to allow; but that it is distinguished from every other article of that class of substances, simply requires an examination of its powers. We perceive a material difference in its operation from sedatives in general; it does not even in small doses excite those exhilarating effects which are universally ascribed to opium. It is true, that by subduing pain, it produces tranquillity of mind; by soothing the patient's sufferings it excites cheerfulness and comfort; it charms away agony and banishes despair. But in producing these effects it operates very differently indeed from opium and other narcotics; they stimulate in the first instance, and excite the sensorium in an unusual manner; they exercise a very marked influence over the sanguineous and other systems of the body. In fine, there are but few points of resemblance; few assimilating traits by which we can identify them with each other.

It would be trespassing beyond the proposed extent of this essay, to enter upon the examination of all the remarkable properties of this active substance; we will therefore content ourselves with a mere recital of the diseases in which it has been employed with advantage. It may not be unimportant to remark that the evidence in favour of its use is of the most respectable kind, derived from some of the most eminent practitioners of the continent of Europe and Great

Britain. Reference to Magendie's memoir, at one view gives us the history of its application to several cases of phthisis; and Drs. Manzoni and Borda bear ample testimony of its successful employment in consumptive cases; and Drs. Granville and Scudamore, and some other distinguished physicians, declare it to be, beyond all comparison, the most valuable remedy that has ever been made use of in the treatment of phthisis, and certain other complaints.

The medical world is justly sceptical with regard to the powers of new medicines. And when we consider the high but ephemeral reputation of some articles of the *materia medica*, we are warranted in receiving, with great caution the reports of individuals, carried away perhaps by false zeal and enthusiasm.

Dr. Granville, who has had more experience than the other English practitioners in the use of this article, thus expresses himself with regard to its action on a patient exhausted by disease or the previous medical treatment: It exerts, says he, an immediate influence on the nervous system, it gradually diminishes all irritability, checks too rapid a circulation, and calms many of the symptoms of fever; if a dry cough is present it promotes expectoration in the first instance, and subsequently stops the cough itself; the spirits, before exalted, soon feel the quieting impression of the acid; they become subdued: the speech, the countenance, and even the expression of the eyes, assume the character of unusual meekness. There is relief from pain and actual suffering. But he tells us, that in some few cases the sedative effects are much more considerable; that there is in some instances an apparent entire prostration of strength, great lowness of spirits, and unwillingness to move, speak, or take food; life seems suspended; and indeed the patient expresses himself as if only half alive, yet the head and mind remain clear and intelligent; there is a total absence of pain; the heat of the skin is natural, and the pulse continues its course steadily and quietly; and, after some hours, the system returns to its original condition.

From what has been said of this article, it must appear that we cannot limit its application to any particular disease, but

that its use is as extensive as the indications of morbid irritability. I have had myself no experience with this acid; but Dr. Cullen informs me that a fit subject fell under his notice, whom he treated as directed by Magendie; the case was, however, desperate; disorganization of the lungs had taken place, and a palliative system was alone pointed out. The prussic acid answered every object in view, and corresponded exactly with the histories of Magendie and Granville. The patient died, it is true; but he expressed great gratitude for the mitigation of his sufferings. The acid was prepared according to Vauquelin's process: two drops dissolved in a table spoonful of mucilage were at first given twice a day, but gradually increased to sixteen in the twenty-four hours.

BIOGRAPHY.

A Tribute to the Memory of the late President of the Literary and Philosophical Society of Manchester. By WILLIAM HENRY, M.D., F.R.S., &c. &c.

[This account of the life of the late Mr. WILLIAM HENRY was read to the Literary Society of Manchester, in April 1817.]

[From the Quarterly Journal, for October 1819.]

THE following tribute to the memory of the late President of the Literary and Philosophical Society of Manchester has been drawn up in compliance with a request, expressed to the writer from the chair, at an early meeting during the present session. It would, on some accounts, have been more satisfactory to him, that the office should have fallen into other hands. But, conceiving a compliance with the requisition to be a duty, which he was not at liberty to decline, he has endeavoured to execute it with all the impartiality and fidelity in his power; and he trusts to the candor of the Society for that share of indulgence, which he may reasonably claim, in speaking of one to whom he was so nearly allied.

The late Mr. Henry was descended from a respectable family, which for several generations, had resided in the county of Antrim. His paternal grandfather commanded a company of foot in the service of James the Second; and during the disturbed times, which, in Ireland, succeeded the revolution, was shot by an assassin in his own garden. The father of Mr. Henry, then an infant scarcely a year old, was taken under the generous protection of a neighbouring nobleman * who continued it to him during the remainder of his life; and, after being educated in Dublin at his lordship's expense, was brought over by him into Wales, when he had nearly attained the age

* Viscount Bulkley.

of manhood. Having there, a few years afterwards, married the daughter of a respectable clergyman of the establishment, they sought the means of support by jointly engaging in the education of females, and for many years conducted a respectable boarding-school, first at Wrexham in North Wales, and afterwards in Manchester.

It was at the former place that Mr. Henry was born, on the 26th of October, O. S. in the year 1734. For some years he remained under the tuition of his mother, who was admirably fitted for the task, and of whom he was always accustomed to speak with the warmest affection and gratitude. At a proper age, he was sent to the Grammar school of Wrexham, at that time in considerable repute. There he was fortunate in having for his first classical instructor, the Rev. Mr. Lewis, whose virtues and talents are the subject of an elegant Latin epitaph, copied by Mr. Pennant into his *Tour through Wales*.* At this school Mr. Henry remained for several years, and made such proficiency in his classical studies as to have attained the foremost station, with the exception only of Mr. Price, who was afterwards well known as the keeper of the Bodleian Library in the University of Oxford.

The inclination of Mr. Henry from early life led him to the church; and it was determined that, on leaving school, he should remove to Oxford. The day of his departure was accordingly fixed, and a horse was provided for the journey. But as the time drew near, his parents, who had a numerous family, and were far from being in affluent circumstances, grew discouraged at the prospect of expenses that were unavoidable, and at the uncertainty of eventual success. While they were thus hesitating, Mr. Jones, an eminent apothecary, of Wrexham, decided the point, by proposing to take Mr. Henry as an apprentice; and to this measure, though deeply feeling the disappointment of long indulged hopes, he could not deny the reasonableness of assenting. With Mr. Jones he continued, till that gentleman died suddenly from an attack of gout, when he was articulated, for the remainder of the term, to a respectable apothecary at Knutsford in Cheshire.

In neither of these situations did Mr. Henry enjoy any extraordinary opportunities of improvement. The only book which he remembered to have been put into his hands, by either of his masters, was the Latin edition of *Boerhaave's Chemistry*, in two volumes quarto, a work which, whatever may have been its merits, was certainly not calculated to present that science to a beginner under a fascinating aspect. His reading was, therefore, entirely self-directed; and, by means of such books as chance threw into his way, he acquired a share of knowledge, creditable both to his abilities and his industry.

At the expiration of his apprenticeship, he engaged himself as principal assistant to Mr. Malbon, who then took the lead as an apothecary at Oxford. In this situation, he was treated by Mr. Malbon with the indulgence and confidence of a friend, and his time was chiefly spent in visiting patients of the higher class, a majority of whom were members of the University. Among the students at Oxford, were several who recognised Mr. Henry as a former associate, and who, though holding the rank of gentlemen-commoners, renewed their acquaintance with him, and offered him the most friendly countenance. His leisure hours were, therefore, spent most agreeably and profitably in the different colleges; and his taste for literary pursuits were encouraged and confirmed. At Oxford he had an opportunity of attending a course of anatomical lectures, in which the celebrated John Hunter, then a young man, was employed as a demonstrator.

From Mr. Malbon, who was become affluent, Mr. Henry received a strong mark of esteem and confidence in the offer of a future partnership. To have accepted this it would have been necessary that he should have qualified himself to matriculate, which would have required the completion of a residence of seven years. But other views in life, which were inconsistent with so long a season of expectation, induced him to decline the proposal; and, in the year 1759, he settled at Knutsford, where he soon afterwards married. After remaining five years at this place, he embraced the opportunity of succeeding to the business of a respectable apothecary in Manchester; where he continued, for nearly half a century, to

be employed in medical attendance for the most part on the more opulent inhabitants of the town and neighbourhood.

Soon after Mr. Henry's settlement in Manchester, the late Dr. Percival removed to the same town from Warrington. That eminent physician was early inspired with the same ardent zeal for the cultivation of professional and general knowledge, which afterwards so much distinguished him. Between Dr. Percival and the subject of this memoir, congeniality of taste and pursuits led to a frequent intercourse; and the moral qualities of both cemented their connexion into a friendship which continued, without interruption, until it was terminated by the death of Dr. Percival, in 1804. It was about the same period, that he formed an acquaintance with that excellent man, and upright magistrate, the late Mr. Bayley, of Hope-Hall, and much of the happiness of his future life was owing to the mutual esteem and confidence, and to the frequent intercourse, which continued to exist between them for more than thirty years.*

During his apprenticeship, Mr. Henry had manifested a decided taste for chemical pursuits, and had availed himself of all the means in his power, limited as indeed they were, to become experimentally acquainted with that science. This taste he continued to indulge after his settlement in life; and, after having made himself sufficiently master of what was ascertained in that department of knowledge, he felt an ambition to extend its boundaries. In the year 1771, he communicated to the Royal College of Physicians of London, "*An Improved Method of Preparing Magnesia Alba*," which was published in the second volume of their Transactions. Two years afterwards it was reprinted, along with essays on other subjects, in a separate volume, which was dedicated by Mr. Henry to his friend Dr. Percival.

The calcination of magnesia had, at that time, been practised only in connexion with philosophical inquiries. Dr. Black, in an essay which is still, perhaps, not surpassed in chemical philosophy as an example of inductive investigation, had fully

* An interesting biographical sketch of Mr. Bayley, written by Mr. Percival, appeared in one of the volumes of the *Monthly Magazine* for the year 1802.

established the differences between magnesia in the common and in the calcinated state; but he does not appear to have made trial of the pure earth as a medicine, though several inconveniences, from its use in the common form, had long before been pointed out by Hoffman.* On this subject Mr. Henry's claims extend to the free disclosure of his improvements; to the early and strenuous recommendation of the medicinal use of pure magnesia; and to the discovery of some of its chemical agencies. It is but justice to him to state that his recommendation of its employment as a medicine was perfectly disinterested; for it was not till his work was printed, and on the eve of issuing from the press, that the preparation of magnesia for sale was suggested to him by a friend, in a letter relating to the intended publication, which is still preserved as a part of his correspondence. Before carrying this suggestion into effect, he thought it proper to consult Sir John Pringle, Sir Clifton Wintringham, Dr. Warren, and some other leading members of the College of Physicians, as to their opinion of the propriety of the measure; and he did not adopt it until those gentlemen had each declared it to be not more advisable on his own account, than on that of the public.

Soon after the publication of the small volume of essays, Mr. Henry found himself involved in a controversy, arising out of some remarks in the appendix, respecting which, as the subject was of temporary interest, it is unnecessary to enter into particulars. It is sufficient to state that the accuracy of some of his experiments, which had been called in question, was confirmed by the concurrent testimony of Dr. Percival and Dr. Aikin; and that the chemical properties, first ascertained by him to belong to pure magnesia, were considered, by Bergman and by Macquer, as worthy of being incorporated into their respective histories of that earth.

It was probably in consequence of the publication of these inquiries, that Mr. Henry was admitted into the Royal Society of London, of which he became a Fellow in May, 1775. The persons most active in promoting his election, were Sir John Pringle and Dr. Priestley; and he had the advantage not

only of the vote, but of the favourable influence of Dr. Franklin, who happened at that time to be in London. Several years afterwards, the same venerable philosopher, when in the 81st year of his age, presided in the meeting of the American Philosophical Society, at which Mr. Henry was elected a member, and again honoured with his suffrage.*

The writings of the celebrated Lavoisier were introduced by Mr. Henry, to the notice of the English reader in 1776. The earliest work of that philosopher was a volume, consisting partly of an historical view of the progress of pneumatic chemistry from the time of Van Helmont downwards; and partly of a series of original essays, which are valuable as containing the germs of his future discoveries. To this work, Mr. Henry added, in the notes, occasional views of the labours of contemporary English chemists. A few years afterwards he translated, and collected into a small volume, a series of *Memoirs*, communicated by Mr. Lavoisier to the Paris Academy of Sciences, when the views of that philosopher, respecting the antiphlogistic theory of chemistry, were more fully unfolded. In undertaking the translation of these works, he was influenced by a desire to place within the reach of English readers, among whom the knowledge of the French language was then confined to comparatively few, the pleasure and conviction which he had himself derived from these beautiful models of philosophical inquiry.

Notwithstanding the large share of professional employment to which Mr. Henry had now attained, he still continued to engage frequently in experimental pursuits, the results of which, at this time, were communicated to the world, chiefly through the publications of his friends Dr. Priestly and Dr. Percival. Of these, the most important were some experiments on the Influence of Fixed air in Vegetation, by which he endeavoured to show that though fixed air is injurious, when unmixed, to the vegetation of plants, yet that when mingled in small proportion with common air, it is favourable to their growth and vigour. The facts established by this enquiry, were com-

* This circumstance is stated in a letter from Dr. Rush to Mr. Henry, dated Philadelphia, 29th July, 1788.

municated to Dr. Priestley; and it is creditable to the candor of that distinguished philosopher, that he was anxious to make them public, not only for their general merit, but because in one or two points the results disagree with his own. "I am much pleased," Dr. Priestley replies, "with the experiments mentioned in your letter, and if you have no objections, shall be glad to insert the greater part of it in my Appendix, which I am just sending to the printer's. I the rather wish it, as a few of the experiments terminate differently from those that I shall publish, and I wish to produce all the evidence I can come at on both sides. The other experiments are very curious and will give much satisfaction."* The investigation was afterwards resumed by Mr. Henry, and made the subject of a paper, which is printed in the second volume of the *Memoirs of this Society*.

The occasion of Mr. Henry's next appearance, as the author of a separate work, arose out of an accidental circumstance. He had found that the water of a large still tub was preserved sweet for several months by impregnating it with lime, though, without this precaution, it soon became extremely putrid. This fact suggested to him an eligible method of preserving water at sea;† but as lime water is unfit for almost every culinary purpose, some simple and practicable method was required of separating that earth from the water, before being applied to use. This he ascertained might be accomplished at little expense by carbonic acid, the gas from a pound of chalk and 12 ounces of oil of vitriol being found sufficient for the decomposition of 120 gallons of lime water.‡ The only difficulty was in the mode of applying the gas on the large scale; but this was overcome by the contrivance of an apparatus, which Mr. Henry described in a pamphlet dedicated to the Lords of Admi-

* Letter from Dr. Priestly to Mr. Henry, dated Jan. 5, 1777.

† Dr. Alston of Edinburgh appears, however, to have been the first who proposed impregnation with lime, as a mean of preventing the putrefaction of water; and to precipitate the lime, he suggested the use of carbonate of magnesia.

‡ The water, however, for which these proportions were sufficient, could not have been completely charged with lime, for fully saturated lime-water would have required for decomposition nearly three times that quantity of chalk and oil of vitriol.

ralty. The proposal, in consequence of the zealous personal exertions of Mr. Wedgwood, who was then in London, met with due attention from the Commissioners for victualling his majesty's ships. The chief obstacle to its adoption in the navy was an apprehension, probably well grounded, that persons would scarcely be found on ship-board, possessing sufficient skill for conducting the process successfully. Since that time, the preservation of water at sea has been accomplished by the simple expedient of stowing it in vessels constructed or lined with some substance, which is not capable of impregnating water with any putrescible ingredient; for good spring water, it is well known, contains essentially nothing that disposes it to putrefaction.

The philosophical pursuits of Mr. Henry, not long after this period, received an additional stimulus by the establishment of the Society to which these pages are addressed; and by his anxious desire to fulfil his duties as a member of it. To him, on its being first regularly organized in the winter of 1781, was confided the office of one of the Secretaries. At a subsequent period, he was advanced to the office of Vice-President, and in the year 1807, on the vacancy occasioned by the death of the Rev. George Walker, F.R.S., he received from the Society, and retained during the rest of his life, the highest dignity which it had to bestow.

The "*Memoirs of Albert de Haller*," which were published by Mr. Henry in 1783, and dedicated to this Society, were derived partly from a French *Eloge*, and partly from information communicated by the late Dr. Foart Simmons. A more complete view of the life and acquirements of that extraordinary man might have been collected, at a subsequent period, from other publications of the same kind, which were addressed to different learned societies on the continent. In one respect, Mr. Henry appears to have taken too favourable a view of the character of Haller, in ascribing to him gentleness of disposition; for that illustrious, and in the main, excellent person, seems to have been a man of quick passions, and not sufficiently reserved in the expression of them; as may be gathered from his controversy with Dr. Whytt of Edinburgh.

Haller is represented, also, by his biographer, as afflicted with the personal defect of weak eyes; which, from a passage in his *Physiology*,* appears not to have been correct. “*Aquæ puræ,*” he says, “*qua ab anno ætatis 18 sola utor, tribuo, quod post tot in fulgendo sole susceptos microscopicos labores, omnibus sensibus, et oculis potissimum, non minus valeam, quam puer valui.*”

During the long season of Mr. Henry's activity as a member of this Institution, his communications to it were very frequent. Many of these were intended only to excite an evening's discussion, and having served that purpose were withdrawn by their author, but the number is still considerable, which are preserved in the Society's published volumes. As might be expected, they are of various degrees of merit, but there are among them two papers, which have contributed greatly to his reputation as a chemical philosopher†.

* Tom. vi. p. 240 Edit. 2. Lausannæ.

† The following is a list of Mr. Henry's Papers, that are dispersed through the printed Memoirs of this Society:

In Vol. I (1.) An Essay on the Advantages of Literature and Philosophy in general, and especially on the Consistency of Literary and Philosophical with Commercial Pursuits.

(2.) On the Preservation of Sea Water from Putrefaction, by means of Quicklime.

(3.) On the Natural History and Origin of Magnesian Earth, particularly as connected with those of Sea Salt and Nitre, with Observations on some Chemical Properties of that Earth, which have been hitherto unknown or undetermined,

In Vol. II. (1.) Experiments on Ferments and Fermentation, by which a Mode of exciting Fermentation in Malt Liquors, without the aid of Yeast, is pointed out; with an attempt to form a new Theory of that Process.

(2.) Observations on the Influence of Fixed Air on Vegetation, and on the probable Cause of the Difference in the Results of various Experiments made for that purpose.

In Vol. III. (1.) Observations on the Bills of Mortality for the towns of Manchester and Salford.

(2.) Case of a Person becoming short-sighted in Advanced Age.

(3.) Considerations relative to the Nature of Wool, Silk, and Cotton, as Objects of the Art of Dyeing; on the various Preparations and Mordants requisite for these different substances; and on the Nature and Properties of Colouring Matter—Together with some observations on the theory of Dyeing in general, and particularly the Turkey-Red.

New Series, Vol. II Remarks on Mr Nicholson's Account of the Effects produced at Swinton by a stroke of Lightning. And

The Essay on Ferments and Fermentation is valuable, not for the theoretical speculations which it contains, for these have been superseded by subsequent discoveries; but for a few facts of considerable importance. It was at that time believed that the infusion of malt, called *wort*, could not be made to ferment, without the addition of yeast, or barm; but Mr. Henry discovered that wort may be brought into a state of fermentation, by being impregnated with carbonic acid gas. By a fermentation thus excited, he obtained not only good beer, but yeast fit for the making of bread; and from separate portions of the fermented liquors, he procured also ardent spirit and vinegar, thus proving that the fermentative process had been fully completed. He found, moreover, that flour and water, boiled to the consistence of thin jelly, and impregnated with carbonic acid in a Nooth's machine, passed into fermentation, and by the third day had assumed the appearance of yeast, for which it served as a tolerable substitute in the baking of bread.

The other memoir, which is distinguished by its value and importance, is entitled, "Considerations relative to the Nature of Wool, Silk, and Cotton, as Objects of the Art of Dyeing; on the various Preparations and Mordants requisite for these different Substances; and on the Nature and Properties of Colouring Matter."

After having given a general view of the history of the art of Dyeing, Mr. Henry, in this elaborate essay, examines the theories that had been framed to account for the various facility and permanency with which different substances attract colouring matter. He demonstrates the futility of these hypotheses, that explained the facts by supposed peculiarities of mechanical structure in the materials to be dyed; and suggests the probability, that the unequal powers of absorbing and fixing colouring matter, manifested by wool, silk, linen, and cotton, depend on the different attractions inherent in those substances as chemical compounds, for the various colouring ingredients. All the preparatory operations, though differing

And a Paper, printed in this volume, entitled, Memoirs of the late Charles White, Esq. F.R.S., chiefly with a Reference to his Professional Life and Writings.

from each material, have, he apprehends, one common object, viz., the removal of some extraneous matter, which, being already united with the substance to be dyed, prevents it from exerting its attraction for colouring matter. The ultimate object of these preliminary steps, he states to be the obtaining a white ground, that may enable the colours to display the full brilliancy of their several tints. To explain the preparation of cotton for the Turkey-red dye, he endeavours to prove that cotton requires, for this purpose, to be approximated, in composition, to the nature of an animal substance. He next offers a classification of the *Materia Textoria*, and some general speculations on the nature of colouring matter.

In the second part of the Essay, Mr. Henry investigates the mode of action of those substances which, though themselves destitute of colour, are important agents in the processes of dyeing. Substances of this kind had received, from the French dyers, the name of *Mordants*, because it was imagined that they corroded and removed something, which mechanically opposed the entrance of the colouring matter into the pores of the material to be dyed. To destroy this erroneous association, Mr. Henry proposes that the word *basis* should be substituted, as a general term, to denote every substance, which having an affinity both for the colouring matter, and for the material to be dyed, is capable of serving as an intermedium between the two; and that a specific epithet should be added, to distinguish each particular variety. In this essay, Mr. Henry, for the first time, explained the true nature of the liquor which is employed for affording the aluminous basis, prepared by mixing the solutions of alum and of sugar of lead. This liquor he showed to be essentially a compound of pure clay of alumine with acetic acid; and its superiority over a solution of common alum, for yielding the earthy basis in dyeing, he ascribes partly to the less affinity of the acetic, than of the sulphuric, for alumine, and partly to the greater volatility of the acetic acid, when exposed to a moderate increase of temperature. The remainder of the paper is chiefly occupied with the details of the operations then practised for dyeing Turkey-red; with a theory of the process, and with a general view of the mode of action of the individual mordants or bases.

The methods of dyeing Turkey-red have been since much improved and simplified, though its theory is, even yet, far from being well understood. But the opinions inculcated by Mr. Henry respecting the action of mordants, evince a remarkable superiority to the prejudices with which he found the subject encumbered, and are indeed those which are still held by the latest and best writers on the principles and practice of dyeing.

In the year 1783, an institution arose out of this society, which had great merit, not only in its plan and objects, but in the ability exerted by the several persons, who were concerned in their fulfilment. It was destined to occupy, in a rational and instructive manner, the evening leisure of young men, whose time during the day, was devoted to commercial employments. For this purpose regular courses of lectures were delivered on the belles lettres, on moral philosophy, on anatomy and physiology, and on natural philosophy and chemistry. Mr. Henry, assisted by a son, whose loss he had afterwards to deplore, and whose promising talents and attainments obtained for him, at an early period of life, a mark of the approbation of this Society,* delivered several courses of lectures on chemistry to numerous and attentive audiences. From causes which it is not easy to trace, but among which, I believe, may be reckoned a superstitious dread of the tendency of science to unfit young men for the ordinary details of business, this excellent institution fell into decay. Mr. Henry, however, continued his lectures long after its decline, until deprived of the services of his son, by the prosecution of views at a distance, when he found that his own leisure was not, of itself, adequate to the necessary preparations.

That the schemes of establishing in Manchester a College of Arts and Sciences (for so it was entitled,) was not a visionary project, but one which appeared feasible and promising to men of sense and knowledge at a distance, is shewn by the following extracts from letters addressed to Mr. Henry, in

* See Dr. Percival's eloquent address to Mr. Thomas Henry, junior, on presenting to him the silver medal of the Society.—*Memoirs of the Society*, Vol. II. page 513.

reply to his communication of the plan. "An attempt of this kind," the late Dr. Currie of Liverpool observes, "I think most praise worthy; and for this, however the matter may terminate, the projectors will always be entitled to public favour and esteem. It is a bold enterprise, and of course in some degree doubtful. One thing appears to be probable;—that if the business is taken up as it ought to be by the public, you will soon find the propriety of extending your plan, so as to make it embrace every object of general education." Mr. Wedgwood strongly expressed his approbation of the undertaking. "The plan of your college," he says, "I think an excellent one, and from the populous and commercial state of your town—from the apparent utility of the Institution—from the elegance and propriety with which it is announced—and from the known characters of the gentlemen who are engaged in it, I can scarcely entertain a doubt of its meeting with success." Greater perseverance would perhaps have gradually softened, and finally subdued, the prejudices that seem to have existed against the union of commercial with literary or philosophical pursuits,—an union which, under proper regulation, adorns and dignifies the character of the merchant, without, it may be hoped, diminishing his usefulness, or interfering with the prosperous management of his affairs.

Besides the lectures on the general principles of chemistry, Mr. Henry delivered a course on the arts of bleaching, dyeing, and calico-printing; and to render this course more extensively useful, the terms of access to it were made easy to the superior class of operative artisans. It was at this period, that the practical application was made in France of a philosophical discovery to one of the arts which Mr. Henry was engaged in teaching, that shortened, by several weeks, the duration of its processes. In 1774, Scheele a Swedish chemist, distinguished by the number and great importance of his contributions to chemical science, discovered, in the course of some experiments on magnesia, the substance known successively by the names of dephlogisticated marine acid, oxy-muriatic acid, and chlorine. During several years afterwards, its properties were not applied to any practical use, until its power of discharging vegetable colours suggested to M. Berthollet, of

Paris, its employment in the art of bleaching. The first successful experiments with that view were made by M. Berthollet in the year 1786, and, with a liberality which confers the highest honour upon him, he freely communicated his important results, not only to his philosophical friends, but to those who were likely to be benefited by them in practice. Among the former was Mr. Watt of Birmingham, who happened at that time to be in Paris, and who was the first person in this country to carry the discovery into effect, by bleaching several hundred pieces of linen by the new process, at the works of a relative near Glasgow. Mr. Henry also, having received an indistinct account of the new method, but not knowing precisely in what it consisted, immediately set about investigating the steps of the operation; and in this he was fortunate enough to succeed. Soon afterwards, an attempt was made by some foreigners, who themselves had acquired their information from Berthollet, to turn the process to their own advantage, by obtaining a patent; and having failed in that, by applying for a parliamentary grant of an exclusive privilege of using it for a certain number of years. Against the former, a strong memorial, which is now before the writer, was presented by Mr. Henry to the Attorney and Solicitor-General; and effectual opposition was made to the latter, by a public meeting of the inhabitants of Manchester, on the ground that the whole process had been successfully carried into effect by Mr. Watt, Mr. Henry, and Mr. Cooper.*

Having satisfied himself of the practicability and advantages of the new method of bleaching, by carrying it on upon a scale of sufficient extent, Mr. Henry prepared to embark in a much larger establishment for the purpose. The connexion, however, which he entered into with this view, having disappointed his just expectations, and the further prosecution of it being inconsistent with his professional employments, he abandoned the project, and contented himself with imparting the knowledge he had gained to several persons, who were already ex-

* The reader, who is interested in the history of the introduction of chlorine and its compounds into use in bleaching, is referred to a note in Dr. Brewster's *Edinburgh Encyclopædia*, ART. BLEACHING; and to Dr. Thomson's *Annals of Philosophy*, Vols. 6 and 7.

tensively engaged in the practice of bleaching, by the then established methods.

Mr. Henry had now reached a period of life, when the vigour of the bodily powers, and the activity of the mind, begin, in most persons, to manifest a sensible decay. From this time, however, though he did not embark in new experimental inquiries, yet he continued for many years to feel a warm interest in the advancement of science, and to maintain an occasional correspondence with persons highly eminent for their rank as philosophers, both in this and other countries.* His medical occupations had greatly increased, and, for a further interval of fifteen or twenty years, he had a share of professional employment, which falls to the lot of very few. This, and the superintendence of some chemical concerns, prevented him from attempting more than to keep pace with the progress of knowledge. He was in no haste, however, to claim that exemption from active labour, to which advanced age is fairly entitled, and it was not till a very few years before his death, that he retired from the exercise of the medical profession.

The summers of the years 1814 and 1815 were spent by Mr. Henry in the country, a mode of life, which, now that his season of active exertion was passed, was peculiarly suited to him, not only for the tranquil retirement which it afforded, but by its enabling him to indulge that sensibility to the charms of rural scenery, which can, perhaps, only exist in a pure and virtuous mind. His perception of these pleasures was at no period more lively, than after he had entered his 81st year. In a note, addressed to the writer of these pages, in the autumn of 1815, he describes, in animated language, one of the events, which so agreeably diversify the face of nature in the country. "Yesterday," he says, "we had one of the most beautiful appearances in the garden I ever witnessed. Every

* A considerable collection of letters to Mr. Henry from persons of this description has been preserved; but the subjects of them have, for the most part, been long ago brought before the public by their respective writers. The letters are chiefly valuable to the family of the deceased, as unequivocal proofs of the respect and esteem felt towards him, by those who were best qualified to judge of his merits. Many of them are from learned foreigners, with whom he had enjoyed opportunities of personal intercourse during their visits to Manchester.

leaf—every petal—every projecting fibre—was beset with a minute globule of water, and when the sun shone upon the flowers and shrubs, they seemed as if studded with myriads of brilliants. The gossamer, too, with which the hedges were covered, was adorned with the same splendid appendages. The cause," he adds, "of this disposition of moisture must, I suppose, have been electrical."

The winter of the year 1815, which Mr. Henry passed in Manchester, was a season of greater suffering than was usual to him; for though of a delicate constitution, yet he happily, even at this advanced life, enjoyed an almost entire exemption from painful diseases. During this winter, he was much distressed by cough and difficult breathing, and his bodily strength rapidly declined. In the spring of the following year, he returned into the country, but not to the enjoyments which he had before derived from it. He was unable to take his customary walks, and was oppressed by feelings, which induced him to look forward to the close of life, but with calm and dignified resignation. The event, which he had anticipated, took place on the 18th of June, 1816, when he had nearly completed his 82d year.

In estimating the intellectual character and attainments of the subject of this memoir, it is proper to revert to a period, several years remote from the present, but still within the perfect recollection of many to whom these pages are addressed. At that time, the quality of Mr. Henry's mind which was perhaps, most conspicuous, was a readiness of apprehension, that enabled him to acquire knowledge with remarkable facility. To this was joined a quickness in his habits of association, that peculiarly fitted him to perceive those analogies which, in chemical investigations, were chiefly relied upon as leading to the discovery of truth, before it was sought to be established on the firmer basis of an accurate determination of quantities and proportions. Without claiming for Mr. Henry the praise of great original genius, we may safely assert for him a very considerable share of that inventive talent, which is commonly distinguished by the term *ingenuity*. This was especially displayed in the neatness and success with which he adapted to

the purposes of experiment, the simple implement that chance threw in his way; for it may be proper to observe that, at no period of his life, was he in possession of a well-furnished laboratory, or of nice and delicate instruments of analysis or research. With these qualifications, he united a degree of ardour in his pursuits, which enabled him to triumph over obstacles of no trivial amount. And when it is considered that his investigations were carried on, not with the advantages of leisure, ease, and retirement, but amidst constant interruptions, and with a mind harassed by frequent and painful anxieties,—it will be granted, that he accomplished much more than might have been expected, from one so little favoured by external circumstances.

The acquirements of Mr. Henry are not limited to that science in which he obtained distinction. It was the habit of his mind, when wearied by one occupation, to seek relief, not in indolent repose, but a change of objects. In medical knowledge, he kept pace with the improvements of his time, and he occasionally by original publications,* contributed to its advancement. He had a share of general information, and a flow of animal spirits, that rendered him an instructive and agreeable companion. To the rich sources of enjoyment, which are opened by the productions of the fine arts, he was extremely sensible, not so much from an acquaintance with critical rules, as from a natural and lively susceptibility of those emotions, which it is the object of the poet and the artist to excite. By the native strength of his memory, unassisted by any artificial arrangement, he has acquired a knowledge of history, remarkable for its extent and precision; he was always eager to discuss those questions of general policy, which are to be decided, partly by an appeal to historical evidence, and partly by a consideration of the nature of man, and of his claims and duties as a member of society. No representation of him would, indeed, be complete, that failed to notice the animation with which he entered into arguments of this kind, or the zeal and constancy with which he defended his political opinions,—opinions which, in him, were perfectly disinte-

* Chiefly in the periodical Journals, and in the Transactions of some Medical Societies to which he belonged.

rested and sincere, but which perhaps disposed him to allow more than its due weight to the aristocratical part of our mixed government. It would be unjust to him, however, not to state, that no man could more cordially disapprove, or more unreservedly condemn, every undue exertion of power; or could more fervently desire the extension of the blessings of temperate freedom to all mankind. It was this feeling that led him to use his strenuous exertions as a member of one of the earliest Societies for procuring the abolition of the African Slave Trade; and when the great object was at length accomplished, he was affected with the most lively joy and gratitude on the downfall of a traffic, which had long been a disgraceful stain on our national character.

Of his moral excellencies, there can be no inducement to offer an overcharged picture to a Society, by many of whose surviving members he was intimately known and justly appreciated. Foremost among the qualities of his heart, was a warmth of generous emotion, which evinced itself in an enthusiastic admiration of virtue; in an indignant disdain and unqualified reprobation of vice, oppression, or meanness; and in the prompt and unrestrained exercise of the social affections. In temper, he was frank, confiding, and capable of strong and lasting attachments; quick, it must be acknowledged, in his resentment; but remarkably placable, and anxious, whenever he thought he had inflicted a wound, to heal it by redoubled kindness. No man could be more free from all stain of selfishness; more moderate in his desire of worldly success; or more under the influence of habitual contentment. This was in a great measure the result of his having early weighed the comparative value of the different objects of life, and his steady and consistent pursuits of knowledge and virtue, as the primary ends of an intelligent being.

In very advanced age, though his body was enfeebled, his mind retained much of that wholesome elasticity and vigour, which always belonged to it. He was still enabled, by the almost perfect preservation of his sight, to spend a great portion of every day in reading; but, at this period, he derived greater pleasure from works of literature, than from those of science, and especially from his favourite study of

history. During the winter immediately preceding his death, besides several standard historical works, he read with avidity one which had been recently published;* and entered into a critical examination of its merits, with a strength of memory and judgment, that would not have discredited the meridian of his faculties. In his moral character, no change was observable, except that a too great quickness of feeling, of which he had himself been fully conscious, was softened into a serene and complacent temper of mind, varied only by the occasional glow of those benevolent feelings, which continued to exist in him, with unabated ardour, almost to his latest hour. He still continued to receive great pleasure from the society of the young; and to them he was peculiarly acceptable, from the kindness and success with which he studied to promote their rational employments. It was his constant habit to take a cheerful view of the condition of the world; and on all occasions, when the contrary opinion was advanced, to assert the superiority of the times in which he had grown old, over the season of his youth; not only on the unquestionable ground of an increased diffusion of knowledge, but on that of the wider spread of virtuous principles, and the more general prevalence of virtuous habits.

Without encroaching on topics, which are wisely forbidden by the rules of this Society, it may be permitted to me to state, that Mr. Henry was from inquiry and conviction, a zealous advocate of Christianity.—About the middle period of his life, a change of opinion led him to separate from the established Church, to whose service he had early been destined, and to join a congregation of Protestant Dissenters. But in discussing differences of religious belief, he was always ready to concede to others that free right of judgment, which he had claimed and exercised for himself; convinced, as he was, that no conclusion to which the understanding may be led, in the honest and zealous search after religious truth, can, without the highest injustice, be made the ground of moral crimination or reproach.

* Dr. Stanier Clark's *Life of James the Second*.

Such is the view of the character of our late President, that has been taken by one, who, in forming it, may be supposed to have been influenced by feelings and recollections, not altogether favourable to an unbiassed exercise of the judgment. That it is coincident, however, with the estimate of others, from whom impartiality may be more reasonably expected, will appear from the following document, which, at the time when it was presented to the Society, declared the sentiments of all those members, who were in the habit of attending its meetings, or of taking an interest in its proceedings.

“ To the Literary and Philosophical Society of Manchester.

“ We, the subscribed, beg leave to present, to the Philosophical Society, a portrait of our President, painted by Mr. Allen, which having been in a public exhibition, has been declared by competent judges, to be not only a correct resemblance, but likewise an elegant production of art. Our wish is, that a suitable place may be assigned to it, in the room where our meetings are held: and that, if approved by the Society at large, it may be inscribed by them as an affectionate tribute of respect and gratitude to a man, universally beloved for his conciliating qualities and private worth, and peculiarly endeared to us, by the relation in which he stands, as one of the very few founders of the Society, whom an indulgent Providence has still spared to us; a Philosopher, to whose talents we owe much of the approbation which the public has bestowed on our labours; and a Member, whose zeal has, for a period of nearly thirty years, been uniformly exerted, in every station, to promote the peace and prosperity of the Institution over which he presides.”

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

On Lunatic Establishments on the Continent of Europe.

[From Dr. H. W. CARTER'S Short Account of Hospitals.]

THE foundations for lunatics appear to have excited an unusual degree of interest in our author, and to that of Amsterdam he assigns a larger space than to all the establishments of Milan, Padua, and Pisa, put together. The medical officers are two physicians and two surgeons; the number of lunatics, fifty men and ninety women; but of the diet, or the funds, we have no account. The governors appear to control the conduct of the keepers with much strictness, and the moral treatment of the patients is pursued with great propriety and attention. We trust we shall not be supposed to advocate the system of severity. Unnecessary severity we deprecate; and all curtailment of the rational comforts of the patients we are decidedly averse from; but we must say, that there is a certain whining cant of humanity in which we hear many persons indulge, a compliance with the suggestions of which, seems much more calculated for the gratification of the visitors than the benefit of the patients.* A constant inspection by the proper authorities will go far to correct abuses, but these inspectorial visits are not of such modern date, or of such exclusively British origin, as we are in the habit of supposing. In the twelfth century, Benjamin, the Jew of Tudela, found a large building at Bagdad, called "the House of Mercy," destined for the reception of lunatics. They were in chains, it is true, but the

* A patient who is quiet the moment a strong iron handcuff is applied, will seriously injure himself or his keepers in a manacle of leather; he knows the inutility of struggling in the first, and he rarely loses hope of escaping from the latter.

magistrates made a monthly visitation of them, and suffered those who had recovered their reason to return to their friends.*

The due occupation of their time is a point of essential utility to lunatics. At the hospital appropriated for their reception at Berlin, the employment of every hour is fixed, and is announced to the patients by signal; each individual is fully occupied, and the strictest discipline is maintained. Perhaps, as has been observed to us by an enlightened friend, this military appropriation of time is not consistent with our English manners and ideas of liberty. This may be the case; but, in some shape or other, we are advocates for the regular employment of lunatics. It has often struck us, that what is true of other hospitals may be said of those for lunatics;—to make them places of recreation and indulgence, which the unfortunate inmates never experienced in their healthy state, is a very likely mode of ensuring their return, after a temporary absence at their own homes and ordinary occupations. There is an ingenious plan adopted at Sonnenstein Lunatic Asylum, near Dresden, an establishment where bodily exercise is looked upon as of great importance in the treatment. By this plan, sluggish patients, who often lounge about the fire all day, and sleep very little at night, are obliged to exert themselves; they are placed in a machine somewhat on the principle of the wheel formerly employed for the reception of turn-spit dogs; they are properly secured, to prevent all injury; after the first step made in this machine, the person within must necessarily continue to move his limbs as in walking. By these means, after some time, fatigue to any extent may be produced, and the patient often falls into a sound and refreshing sleep. Another very useful contrivance, and of great simplicity, is employed at Berlin to restrain furious patients. They are seated in a chair; the hands secured in proper gloves, but their feet not allowed to touch the floor. This prevents all violent motion as effectually as possible. The inventor we know not; the machine, stated to be used near Dresden, is employed by Dr. Piegnitz.

* Benjaminis Itinerarium. 8vo. Lugd. Bat. 1633. P. 69.

Dr. Hosack's edition of Thomas's Modern Practice.

Dr. Hosack has presented to the medical public, through the press of Collins and Co. a new and enlarged edition of that valuable work, Thomas's Modern Practice of Physic, from the sixth London edition.

This edition is considerably extended and improved both by the author and the editor, and will no doubt from its intrinsic merits, command a very extensive circulation, not only among the students, but also the practitioners of medicine in this country, as well as in Europe.

It is indeed a copious and elaborate manual of medical science, and is extended in this edition, including the appendix by the editor, to upwards of one thousand closely printed pages; the prescriptions are given both in Latin and English, and are equally numerous and valuable.

Dr. Thomas has dedicated this edition to Dr. Hosack.

The typographical execution of the work, does credit to the press from whence it issued.



Of the treatment of the Typhus form of Fever. By DAVID HOSACK, M. D. Extracted from the above.

The following extract from a report made to the governors of the New York Hospital, Sept. 1, 1819; giving a statement of the diseases of that institution, and of the treatment adopted, affords additional evidence of the validity of these opinions.

“During the period of my attendance, an ample opportunity has been afforded to the students, resorting to the hospital for instruction, of observing the *typhus form of fever*, which has been unusually prevalent in this city during the last three months, and of noticing the characteristic symptoms of that type of fever as totally distinct from the yellow fever, with which it has been identified by many physicians, under the general and fashionable appellation of *malignant fever*. The students have also witnessed the successful use of

blood-letting and other evacuations in the first stage, and of yeast, porter, snake-root, the vegetable acids, vegetable nourishments, and frequent ablution with tepid vinegar and water in the more advanced stage of that disease, instead of the *mercurial treatment* so generally practised in many parts of the world, and especially in the United States. Indeed, I believe, that the typhus fever of our country, owes much of its malignity to the indiscriminate use of mercury. I say its indiscriminate use, for in some cases of fever, and in many other diseases, it is the physician's only resource. Even in typhus fever, under some peculiar circumstances, it has been prescribed with infinite benefit.

“Of the seventy-six cases of fever, of which forty were cases of typhus attended in many instances with symptoms of the highest malignity, seventy-four have been cured by the means before enumerated, without the prescription of a particle of mercury. Dysentery in like manner was treated by blood-letting, blisters, emetics, and saline cathartics, and a vegetable diet, instead of the customary treatment by calomel. Of twenty-one patients ill of this disease all recovered or were convalescent at the time I ceased to prescribe. Another observation I have frequently made, and which has been verified in the cases that have recently occurred in the hospital, is that those who are cured without mercury, recover in a much shorter time, than those who are treated by the usual mercurial course. This is not all,—they not only have a shorter convalescence, but they recover without that injury to the constitution, and that liability to rheumatism and other inflammatory diseases that we frequently observe in those who have undergone the operation of mercury.”

Dr. Thomson, professor of surgery in Edinburgh, has published during the present year, “An account of the Varioloid Epidemic which has lately appeared in Edinburgh and other parts of Scotland; with Observations on the Identity of Chicken Pox, with modified Small Pox, in a letter to Sir

James M'Grigor." Forming with the appendix a large octavo volume of 400 pages.

Our readers will recollect that his general views on this subject, were published in the thirty-fourth number of the *Eclectic Repertory*, under nearly the same title as the above; and the intention of the present publication seems to be chiefly to support and confirm the opinions then advanced.

We are, from want of time and room, prevented from giving, at present, any thing more than the general result of his observations, so far as they tend to strengthen our confidence in the security obtained by vaccination against the devastating powers of small pox.

He observes that nothing had occurred in the epidemic as far as he had been able to perceive or learn, "to warrant the supposition, that the modifying or preventive powers of vaccination are weakened or exhausted by time." p. 34.

"Of the 310 individuals," he says, "whom I have seen affected with this epidemic, after having gone through the process of vaccination, one only has died; a result, he adds, which to me appears truly astonishing, when I reflect on the general severity of the eruptive fever, or the great diversities in the state of health, and in the constitutional tendencies of the individuals attacked by it; and on the circumstances often so very unfavourable to recovery, in which many of these individuals have been placed." p. 42, 43.

He observes, that there was nothing which occurred in the progress of the epidemic, that had been to him matter of so much surprize, as the number of persons who were affected with small pox for the second time, p. 51; and that a greater comparative mortality had been observed in cases acknowledged to be secondary small pox, than in those who had passed through the process of vaccination, p. 52. His correspondent, Mr. Bryce, also observes on this subject, "that there is on record more instances of persons suffering severely, nay fatally, from what was considered to be a second attack of small pox, than from small pox after what has been considered perfect vaccination." p. 60.

In page 114, Dr. Thomson observes, "every thing which I

learn with regard to the effects produced on the human constitution by the agency of cow pox inoculation tends to convince me, that not only the form and character of subsequent small pox are modified by it, but also that the qualities of the variolous contagion itself are often changed by it, in such a manner as to render this contagion incapable of being readily propagated by inoculation."

Small pox, modified by previous small pox infection, have appeared to me, says Dr. T. to be in general more severe than small pox modified by vaccination, and accordingly the number of deaths has also been comparatively much greater; for while natural small pox in the present epidemic have proved fatal, nearly in the proportion of 1 in 4, and secondary small pox in the proportion of 1 in 25; small pox after vaccination, has proved fatal in one instance only of above 330 cases which have now come under my observation. p. 201, 2.

Dr. Mudie of St. Andrews, in a letter to Dr. Thomson states, that the occurrence of modified small pox after vaccination, had done more to establish the practice in that neighbourhood than any thing that could have happened; "for every one," adds he, "was able to judge for themselves, in comparing the severity of the disease where no previous vaccination had been performed, and the mildness of that disease after vaccination." p. 244.

Dr. Thomson concludes his letter with these emphatic expressions. "I am unwilling that an evidence so favourable to vaccination as that which these pages contain, should be any longer withheld from the public, especially at a time when the small pox appears to be spreading epidemically over a great part of the known world. In this general prevalence of small pox, however, it is gratifying to find that the modifying power of vaccination is every where so triumphantly manifested, and that even its supposed failures, when accurately investigated, have hitherto only tended to confirm, in the minds of the well informed, the confidence which has been placed in the salutary effects of that practice, a practice becoming every day undeniably more worthy of the encouragement and support of those who have it in their power to recommend and enforce it."

Medical Convention.

Address of the Medical Convention of the United States to
their Constituents and Fellow Citizens.

Capitol, City of Washington, January 8, 1820.

Gentlemen—The National Convention for forming a Pharmacopœia is on the eve of terminating its sittings, after bringing the important business, for which it assembled, to a happy and successful close.

It is really a subject of gratulation, both to the profession and to the people, that this work, which has been for two years in a preparatory state, should at length have reached maturity.

The individuals who conceived the design, and the incorporated bodies who furthered it, have the satisfaction of beholding a novel and interesting spectacle—that of the faculty, itself, by a spontaneous effort, and without public summons, or compensation, compiling a *Codex Medicamentarius*, or book of Rules and Directions, for selecting and compounding the articles employed in practice. The whole civilized world may behold a great and growing nation, speaking a similar language, possessing the same general laws, using an uniform denomination of value, and conforming to each other in the rules of preserving health, and of preparing remedies.

We have appointed a committee of five members to superintend the publication of the book we have compiled. It may be expected that they will execute their task with the smallest practicable delay. We recommend it to your perusal and patronage, as a performance upon which we have bestowed great labour, and the best abilities that we possess.

Under conviction, however, that a revision, from time to time, will be necessary, we have provided for the reception, at seasonable periods, of such amendments as experience shall prove to be requisite. The propriety of this arrangement, we trust, will be evident to every considering mind.

In addition to its professional character, we indulge a patriotic hope, that our Pharmacopœia may act as a bond of union, by drawing the inhabitants and governments of our country to a nearer assimilation with each other.

Done in, and by order of the Convention.

THOMAS T. HEWSON, Sec'y.

SAMUEL L. MITCHILL, President.

*Rain that fell since January 1, 1820, including snow and hail,
when melted.*

1820.	Inches.	$\frac{1}{100}$ ths.
January 4 . .	0	.06
10 . .	0	.70
17 . .	1	.20
19 . .	0	.02
22 . .	0	.05
26 . .	0	.10
28 . .	0	.35
	<hr/>	2 $\frac{48}{100}$ inches monthly.
February 3 . .	0	.40
9 . .	0	.60
9 and 10 . .	0	.80
15 . .	0	.20
16 . .	0	.25
24 . .	0	.03
26 and 27 . .	0	.30
	<hr/>	2 $\frac{58}{100}$
March 5 and 6 . .	0	.09
7, 8, and 9 . .	0	.63
9 and 10 . .	2	.42
12 and 13 . .	0	.60
13 . .	0	.20
20 . .	0	.60
27 . .	0	.15
	<hr/>	4 $\frac{69}{100}$
Total rain in Philadelphia		9 $\frac{75}{100}$ in three months.

Vaccination.

By a late ordinance of the City Councils, *one* physician is appointed to vaccinate the Poor, who is also directed to distribute vaccine matter.

The Commissioners of the District of Southwark have also appointed *one* physician to vaccinate their Poor.

The Board of Commissioners of the Northern Liberties have also appointed a physician to vaccinate their Poor.

Statement of Deaths in the Town of Boston, from the 1st of January, 1819, to the 1st of January, 1820; specifying the Sexes, Ages and Diseases of the deceased Persons.

<i>Deaths in each Month.</i>	<i>Males.</i>	<i>Females.</i>	<i>Totals.</i>	<i>AGES.</i>	
January,	29	28	57	Under 1 Year	130
February,	2	30	52	From 1 to 2	91
March,	30	25	55	2 to 5	41
April,	33	17	50	5 to 10	38
May,	31	25	56	10 to 20	49
June,	17	16	33	20 to 30	125
July,	32	20	52	30 to 40	94
August,	41	53	94	40 to 50	72
September,	6	51	113	50 to 60	55
October,	43	40	83	60 to 70	45
November,	44	34	78	70 to 80	33
December,	35	31	66	80 to 90	16
Totals,	419	370	789	Total,	789

Those who died in town amount to	789
In addition to the above, those buried from the Alms-House and the Town's poor, whose ages and diseases are unknown, amount to	192
Still Born,	89
Total,	1070

The above mentioned Deaths were caused by the following Diseases and Casualties, viz.

Apoplexy,	8	Dropsy,	23
Burns or Scalds,	3	Drowned,	13
Cancers,	3	Dysentery,	12
Casualties,	8	Fever, Typhus,	108
Cholera Morbus,	11	Pulmonic,	46
Cholera Infantum,	7	Pleurisy,	3
Consumption,	174	Bilious,	10
Croup,	9	Nervous,	4
Cynanche Trachealis,	8	Scarlatina,	2
Debility,	9	Rheumatic,	6
Dyspepsia,	2	Malignant,	32
Diarrhœa,	1	Worm,	7
Diseases of the heart,	3	Yellow,	1
unknown,	178	Puerperal,	2
by drinking cold water,	2	Carried forward,	695

Brought forward,	695	Phthisis,	1
Fits,	15	Quinsy,	4
Hepatitis	6	Rickets,	1
Hæmorrhage,	5	Scorbutic,	1
Hernia,	1	Scarlatina Anginosa,	2
Ho ping Cough,	3	Spasms,	5
Hydrocephalus Inter.	23	Still Born,	89
Infantile Diseases,	147	Sudden,	13
Influenza,	2	Suicide,	4
Insanity,	3	Suffocation,	1
Intemperance,	11	Teething,	7
Jaundice,	1	Tetanus,	1
Killed in a duel,	1		
Old Age,	29		
Poison,	1		
		Total,	1070

N. B. The above deaths include all who died in Town and in the Alms-House. There were also eighteen deaths in the Hospital on Rainsford's Island, including those who were sent from town and who arrived sick in vessels which were quarantined.

Published by order of the Board of Health,

JAMES ROBINSON, Secretary.

Boston, January 10, 1820.

Statement of Deaths in the City and County of New York, from the first of January, 1819, to the first of January, 1820; specifying the Sexes, Ages and Diseases of the deceased Persons.

<i>Deaths in each Month.</i>	<i>Males.</i>	<i>Females.</i>	<i>Totals.</i>	<i>AGES.</i>			
January,	130	120	250	Under 1 Year			847
February,	136	87	223	From 1 to 2			306
March,	111	88	199	2 to 5			188
April,	129	97	226	5 to 10			103
May,	116	80	196	10 to 20			157
June,	126	81	207	20 to 30			390
July,	144	117	261	30 to 40			383
August,	238	222	460	40 to 50			316
September,	185	174	309	50 to 60			200
October,	163	146	309	60 to 70			135
November,	156	110	266	70 to 80			93
December,	112	108	220	80 to 90			45
				90 to 100			10
				100 to 110			3
Totals,	1746	1430	3176				Total, 3176

The above mentioned Deaths were caused by the following Diseases and Casualties, viz.

Abscess,	13	Hæmorrhage,	9
Aneurism,	2	Hives,	68
Apoplexy,	67	Infanticide,	3
Asphyxia,	3	Inflammation of the Bladder,	2
Asthma,	8	of the Bowels,	43
Burned or scalded,	15	of the Brain,	24
Carbuncle,	1	of the Chest,	74
Cancer,	11	of the Liver,	28
Caries,	2	of the Stomach,	4
Casualties,	23	Insanity,	12
Catarrh	6	Intemperance,	35
Child-L_d,	15	Jaundice,	8
Cholera Morbus,	42	Killed or murdered,	4
Colic,	5	Locked Jaw,	4
Consumption,	577	Lumbar Abscess,	2
Convulsions,	181	Marasmus,	11
Contusion,	2	Measles,	10
Cramp in the Stomach,	7	Menorrhagia,	2
Diarrhœa,	50	Mortification,	17
Drinking Cold Water,	6	Nervous disease,	4
Dropsy,	80	Old Age,	82
in the Chest,	24	Palsy,	40
in the Head,	119	Peripneumony,	28
Drowned,	61	Pleurisy,	14
Dysentery,	219	Pneu. typhodes,	10
Dyspepsia,	3	Quinsy,	10
Erysipelas,	2	Rheumatism,	4
Epilepsy,	9	Rupture,	1
Executed,	1	St. Anthony's Fire,	2
Fever,	66	Scirrhus of Liver,	5
Bilious,	4	Scrofula or King's Evil,	12
Remittent,	16	Scurvy,	1
Hectic,	1	Sore Throat,	11
Inflammatory,	5	Spasms,	9
Intermittent,	3	Spina Bifida,	2
Malignant,	23	Sprue,	22
Puerperal,	4	Still Born,	168
Putrid,	2	Strangury,	1
Scarlet,	5	Sudden,	28
Typhus,	163	Suicide,	27
Flux, infantile,	133	Syphilis,	13
Fracture,	1	Tabes Mesenterica,	137
Frozen,	1	Teething,	31
Gout,	2		
Gravel,	3		
Hæmoptysis,	5	Carried forward,	3013

Brought forward,	3013	Whooping Cough,	. .	55
Tinea Capitis, 1	Worms,	17
Ulcer, 5			
Unknown, 85			
			Total,	3176

Of these, there were:	Men,	895	
	Boys,	851	
	Total Males,	1746	
	Women,	703	
	Girls,	727	
	Total Females,	1430	
			Total,	3176

REMARKS.

The City Inspector respectfully reports to the board, a statement of the deaths in the City and County of New York for the year 1819, amounting to three thousand one hundred and seventy-six, being less by eighty-nine than what had taken place in the preceding year.

The city was unusually healthy until the month of August last, when the diseases peculiar to the warm season proved very fatal to children, particularly the dysentery and infantile flux, and continued their destructive course until moderated by a change of the atmosphere in October. Those two diseases alone, during four months, carried off three hundred and thirteen persons, principally children—considerably exceeding the number that died of those diseases in the same months of the preceding year.

In September, our city was unhappily visited with malignant fever. Its absence for fourteen years had banished our fears, and led us to repose in confidence on our security. Providentially, however, the prompt, intelligent, and energetic measures of our Board of Health, arrested its progress at an early period, and justly claim the gratitude of their fellow citizens. The total number of deaths of this disease was but twenty-three, who were interred within the city, and about twenty in its neighbourhood who had been previously removed by the Board of Health.

It must be highly gratifying to the benevolent mind, and to those whose humane labours have been so long directed to mitigate the ravages of Small Pox, to learn, that there has not been a single case of death by that disease reported in this city within the last year—a disease which has been, for so many ages, a

scourge to every part of the world; and has, at times, been particularly fatal here.

Whilst consumption and fever, generally, occupy a considerable space in these annual returns, it is consolatory to observe, that the former has not increased: and that fever, particularly Typhus, so fatal, so wide spread, and so unyielding to medical skill in Europe, has been much less malignant in this city the present, than in former years.

GEORGE CUMING, City Inspector.

City Inspector's Office, 10th Jan. 1820.

Statement of Deaths, with the diseases and ages, in the City and Liberties of Philadelphia, from the 1st of January 1819, to the 1st of January 1820.

DISEASES.	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 15	From 15 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	From 100 to 110	Total
Apoplexy - - - -	0	0	0	2	1	0	7	14	11	9	6	5	2	0	0	57
Abscess - - - -	0	0	1	1	0	2	4	1	1	1	1	0	0	0	0	12
Atrophy - - - -	17	4	8	2	1	1	0	2	1	4	3	1	0	0	0	44
Asthma - - - -	1	0	1	0	0	0	1	3	1	1	2	3	1	1	0	15
Aphthæ - - - -	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2
Aneurism - - - -	1	0	1	0	0	0	0	0	1	1	0	0	0	0	0	4
Angina Pectoris -	1	0	0	2	1	0	2	4	1	1	1	0	0	0	0	13
Burns - - - -	0	1	4	2	0	0	1	3	3	1	0	0	1	0	0	16
Consumption of the Lungs }	13	4	10	5	9	25	124	116	88	26	19	18	2	0	0	459
Convulsions - - -	134	23	17	5	2	3	7	3	5	0	2	0	0	0	0	201
Casualties - - - -	6	2	0	1	2	2	8	7	6	0	2	2	1	0	0	39
Catarrh - - - -	17	0	4	2	1	0	0	1	0	1	1	2	0	0	0	29
Colic - - - -	3	0	0	0	0	1	2	2	0	2	1	2	0	0	0	13
Cancer - - - -	0	0	1	0	1	0	1	4	1	4	4	1	0	0	0	17
Caries - - - -	0	0	0	1	0	1	0	2	1	0	0	0	0	0	0	5
Cholera Morbus -	147	78	20	1	0	0	4	2	2	3	2	1	0	0	0	260
Decay - - - -	0	1	1	0	1	0	0	0	3	0	3	1	3	0	0	13
Drunkenness - - -	0	0	0	0	0	0	8	8	5	3	0	0	0	0	0	24
Debility - - - -	76	12	6	3	0	0	0	5	5	3	6	7	4	3	0	130
Diabetes - - - -	0	0	0	0	0	0	1	0	1	0	0	0	0	0	0	2
Diarrhœa - - - -	14	11	6	2	2	0	5	8	7	3	3	4	0	1	0	66
Dysentery - - - -	10	10	1	1	0	0	4	2	1	6	1	0	0	1	0	37
Dropsy - - - -	7	8	7	3	7	2	14	18	22	21	15	6	1	0	0	131
in the Head - - -	32	22	22	9	3	1	1	1	1	0	0	0	0	0	0	92
of the Breast - - -	0	2	0	0	0	0	0	2	1	2	0	1	0	0	0	8
Dyspepsia - - - -	0	0	0	0	0	2	0	0	1	2	1	1	0	0	0	7
Drinking cold water	0	0	0	0	0	0	1	1	0	2	0	0	0	0	0	4
Drowned - - - -	0	0	0	3	2	1	8	6	4	1	0	0	0	0	0	25
Erysipelas - - - -	3	0	0	0	0	1	0	1	0	0	0	0	0	0	0	5
Eruptions - - - -	0	0	0	0	0	0	2	1	0	0	0	0	0	0	0	3
Epilepsy - - - -	1	1	2	1	0	0	1	3	4	0	0	0	0	0	0	13
Fever - - - -	5	1	4	2	1	3	5	2	5	4	0	1	0	0	0	33
Typhus - - - -	0	0	0	6	1	7	35	32	17	12	9	6	1	0	0	126
Bilious - - - -	0	2	1	0	3	6	11	13	8	4	0	1	0	0	0	49
Malignant - - - -	0	0	0	1	2	1	5	1	2	1	0	0	0	0	0	13
Intermittent - - -	0	0	0	0	0	0	2	1	2	0	0	1	0	0	0	6
Remittent - - - -	1	6	5	2	0	0	8	13	2	5	1	0	0	0	0	43
Inflammatory - - -	0	1	2	1	0	0	1	1	0	0	0	0	0	0	0	6
Scarlet - - - -	0	0	1	1	0	0	0	0	0	0	0	0	0	0	0	2
Nervous - - - -	0	0	1	0	2	0	2	2	0	0	0	0	0	0	0	7
Hectic - - - -	0	1	0	0	0	0	3	0	0	0	0	1	0	0	0	5
Puerperal - - - -	0	0	0	0	0	0	2	2	0	0	0	0	0	0	0	4
Gangrene and Mor- tification }	5	1	3	3	0	0	1	2	1	2	3	2	2	0	0	25
Gout - - - -	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	1
Hooping Cough - -	55	46	35	14	1	0	0	0	0	0	0	0	0	0	0	151
Hives - - - -	35	20	19	6	0	0	0	0	0	0	0	0	0	0	0	80
Carried over,	586	257	183	82	43	59	281	289	214	125	86	68	18	6	0	2297

	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 15	From 15 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	From 100 to 110	Total
<i>Brought forward,</i>	586	257	183	82	43	59	281	289	214	125	86	58	18	5	0	2297
Hernia - - - -	0	0	0	0	0	0	0	0	1	0	1	1	0	0	0	3
Hæmorrhage - - -	0	0	0	0	0	1	1	0	4	5	0	0	0	0	0	11
Hydrophobia, - -	0	0	0	0	0	0	1	0	0	1	0	0	0	0	0	2
Inflammation of the Brain }	7	3	6	5	0	1	2	7	0	1	1	0	0	0	0	33
of the Lungs - -	30	14	12	3	3	2	9	9	8	7	11	5	2	0	1	116
of the Stomach - -	4	2	0	0	0	1	3	2	2	1	0	2	0	0	0	17
of the Bowels - -	15	3	2	2	2	1	7	11	5	2	1	3	0	0	0	54
of the Liver - -	2	1	0	0	0	0	1	6	4	5	1	0	0	0	0	20
of the Kidneys - -	0	0	0	0	0	0	1	0	1	0	0	1	0	0	0	3
of the Peritonæum	1	0	0	0	1	1	2	2	0	0	0	0	0	0	0	7
Jaundice - - - -	2	0	0	0	0	0	1	1	0	1	0	0	0	0	0	5
Insanity - - - -	0	0	0	0	0	2	6	8	5	5	1	1	0	0	0	28
Lethargy, - - - -	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	1
Locked Jaw - - -	1	0	0	0	1	0	1	0	0	0	0	0	0	0	0	3
Measles, - - - -	20	31	37	15	3	0	2	0	0	0	0	0	0	0	0	108
Old Age - - - -	0	0	0	0	0	0	0	0	0	0	1	12	19	14	6†	52
Prolapsus Ani - -	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Palsy - - - -	1	0	0	1	0	3	2	5	8	0	0	0	0	0	0	20
Pleurisy - - - -	2	0	2	0	0	1	2	2	1	4	0	0	1	0	0	15
Rheumatism - - -	0	0	1	0	0	0	1	0	1	0	1	0	0	0	0	4
Still Born - - -	145	0	0	0	0	0	0	0	0	0	0	0	0	0	0	145
Sore Throat - - -	5	5	6	7	0	0	0	1	1	0	0	1	0	0	0	27
Suicide - - - -	0	0	0	0	0	0	1	0	1	0	1	0	0	0	0	3
Stone - - - -	0	0	0	1	0	0	0	0	0	1	1	0	0	0	0	3
Scrofula - - - -	0	4	5	0	1	1	3	2	0	1	2	0	0	0	0	19
Spasm in the Sto- mach }	0	0	0	0	0	0	1	0	0	1	0	0	0	0	0	2
Small Pox (natural)	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Suffocation - - -	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0	2
Spina Bifida - - -	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Syphilis - - - -	0	0	0	1	0	0	1	2	0	0	0	0	0	0	0	4
Sudden - - - -	5	0	0	0	0	0	5	8	4	6	4	0	1	0	0	33
Teething - - - -	2	8	3	0	0	0	0	0	0	0	0	0	0	0	0	13
Ulcers - - - -	0	0	0	0	0	0	0	1	0	0	0	1	0	0	0	2
Worms - - - -	1	5	4	1	0	0	0	0	0	0	0	0	0	0	0	11
Unknown - - - -	21	0	6	3	0	2	10	8	2	4	1	1	0	0	0	58
<i>Total,</i>	851	334	269	121	54	76	345	364	262	171	113	96	41	20	7	3124

† One person of this number died between the age of 110 and 120.

NOTE. Of the above there were 795 males of twenty years and upwards, 824 under twenty years; of females, 616 of twenty years and upwards, 659 under twenty years; and 230 children, principally under one year, whose sex is unknown.

Since the last annual statement of deaths in this city, about 130 interments have been reported in the burying ground over Schuylkill, near the Market street bridge. This ground, previous to the last session of the Legislature, when it was vested in the Board of Health, had no legal owners, and persons buried there were not reported. It will also be observed that 108 deaths have occurred the last year from Measles, a disease which did not prevail in 1818.

The sextons of some of the burying grounds within the city, who had been found negligent in making complete returns according to the Health Law, have had their delinquency noticed by the Board, and their reports have during the last year, been made more punctually and correctly. These facts will account for the increased number of deaths reported in the present statement.

Deaths in Philadelphia, in each Month of the foregoing period.

	Adults.	Children.	Total.		Adults.	Children.	Total.
January, . . .	120	72	192	October, . . .	103	139	242
February, . . .	106	89	195	November, . . .	102	152	254
March, . . .	141	118	259	December, . . .	96	151	247
April, . . .	117	97	214				
May, . . .	120	84	204	<i>Total,</i> . . .	1411	1713	3124
June, . . .	121	138	259	By order of the Board of Health,			
July, . . .	122	216	338	JOSEPH PRYOR, Clerk.			
August, . . .	136	296	432	<i>Health Office, January 12, 1820.</i>			
September, . . .	127	161	288				

Statement of Deaths in the City of Baltimore, from the first of January, 1819, to the first of January, 1820; specifying the Sexes, Ages and Diseases of the deceased Persons.

<i>Deaths in each Month.</i>	Males.	Females.	Totals.	AGES.	
January,	77	47	124	Under 1 Year	621
February,	60	56	116	From 1 2	252
March,	81	62	143	2 5	129
April,	97	72	169	5 10	147
May,	68	67	135	10 20	291
June,	74	7	121	20 30	357
July,	126	112	238	30 40	191
August,	196	120	316	40 50	117
September,	272	202	474	50 60	71
October,	138	105	243	60 70	39
November,	54	50	104	70 80	35
December,	60	44	104	80 90	27
				90 100	9
				100 110	1
<i>Totals,</i>	1303	984	2287	<i>Total,</i>	2287

The above mentioned Deaths were caused by the following Diseases and Casualties, viz.

Abscess,	1	Croup,	57
Apoplexy,	21	Decay,	88
Asthma,	5	Dropsy,	41
Burns,	2	in the Head, . . .	27
Cancer,	4	Drowned,	31
Casualties,	17	Dysentery,	32
Child-bed,	18	Epilepsy,	4
Cholera Morbus,	258	Fever,	4
Colic,	4	Bilious,	73
Consumption,	272	Inflammatory, . . .	2
Convulsions,	89		
Cramp in the Stomach, . .	3	Carried forward,	1053

Brought forward,	1053	Mortification,	26
Fever, Intermittent,	4	Murdered,	7
Malignant,	350	Old age,	75
Nervous,	2	Palsy,	7
Remittent,	2	Pleurisy,	41
Typhus,	84	Rheumatism,	5
Flux,	5	Scrofula,	2
Fistula,	1	Small Pox,	1
Gravel,	3	Sore Throat,	11
Gout,	1	Spasms,	3
Hæmorrhage,	3	Still Born,	105
Hives,	3	Sudden Death,	16
Jaundice,	2	by Drinking Cold	
Intemperance,	46	Water,	8
Inflammation of the Sto-		Suicide,	3
mach,	1	Syphilis,	3
of the Lungs,	29	Teething,	27
of the Bowels,	1	Unknown,	91
of the Brain,	10	Whooping Cough,	78
Insanity,	1	Worms,	59
Lock Jaw,	2		
Measles,	116	Total,	2287

Of this aggregate of 2287, there were 571 coloured persons.

By order of the Board of Health,

P. REIGART, Secretary.

Statement of Deaths in the City of Charleston, S. C. from the 1st of October, 1818, to the 1st of October, 1819, inclusive; specifying the Sexes, Ages and Diseases of the deceased Persons.

<i>Deaths in each Month.</i>	<i>Males.</i>	<i>Females</i>	<i>Totals.</i>	<i>AGES.</i>			
October,	46	44	90	Under 3 Years			305
November,	34	34	68	From 3 to 10			84
December,	20	26	46	10 to 20			61
January,	36	29	65	20 to 30			188
February,	25	27	52	30 to 40			156
March,	34	27	61	40 to 50			101
April,	38	22	60	50 to 60			78
May,	57	40	97	60 to 70			59
June,	47	38	85	70 to 80			20
July,	42	42	84	80 to 90			27
August,	100	45	145	90 to 100			9
September,	114	59	173	100 to 110			1
October,	46	20	66	110 to 120			3
Totals,	639	453	1092	Total, 1092			

The above mentioned Deaths were caused by the following Diseases and Casualties, viz.

Abscess,	4	Fistula,	1
Accident,	18	Hæmorrhage,	1
Apoplexy,	11	Hooping Cough,	48
Asthma,	16	Inflammation of Brain,	3
Cancer,	4	of Lungs,	4
Catarrh,	21	Insanity,	5
Child-bed,	7	Intemperance,	7
Colic,	5	Liver Complaint,	9
Cholera Morbus,	2	Locked Jaw,†	14
Consumption,	144	Mortification,	4
Convulsions,	47	Old Age,	40
Cramp,	4	Palsy,	7
Croup,	15	Peripneumony,	2
Debility,	91	Pleurisy,	9
Diarrhœa,	53	Rash,	1
Drinking Cold Water,	1	Rheumatism,	3
Dropsy,	71	Rupture,	3
Drowned,	11	Scrofula,	3
Dysentery,	24	Sore-Throat,	5
Erysipelas,	1	Sore Leg,	4
Fever, Bilious,	19	Spasms,	13
Catarrhal,	26	Sudden Death,	4
Country,	8	Suicide,	4
Hectic,	1	Teething,	33
Intermittent,	2	Thrush,	7
Nervous,	16	Unknown,	6
Typhus,	16	Violence,	2
Worm,	35		
Yellow,*	176		
		Total,	1092

Of the above, there were: Males, 639

Females, 453

1092

Of whom there were: Whites, 492

Blacks, 600

1092

The cases of Consumption were generally strangers, who came to Charleston for the benefit of their health.

By order of the Board,

JAMES A. MILLER, Clerk.

Examined and found to be correct.

DANIEL STEVENS, Chairman.

* As the *Yellow Fever* had not ceased its ravages at the time of closing the foregoing statement, those who fell victims to that

† For remarks on *tetanus*, see next page.

calamity on and after the first day of October, 1819, must of necessity be numbered in the next year's report, which will, as it has already done, occasion erroneous and injurious impressions abroad in relation to the healthiness of the climate of Charleston. In their report of Deaths on the first of October, 1817, they state that 232 were caused by *Yellow Fever*—and in their statement of the next year, viz. of the first of October, 1818, they report 38 deaths by the *same disorder*, although not a solitary case of it occurred during the year, 1818. The citizens of Charleston should use their influence to alter an arrangement which operates so injuriously to the character of their city, by commencing their statement, as is done in all other cities of the United States and in Europe, on the *first day of January*, and closing it on the *last day of December*, in each year.

† It is a singular fact, and perhaps worthy of the attention of Medical Gentlemen, that more deaths were occasioned by **TETANUS** or **LOCKED JAW**, in the city of *Charleston*, during the last three years, than occurred in the cities of *Philadelphia*, *New York*, *Baltimore* and *Boston*, during the same period, as is shown by the following abstract:—

Deaths by Tetanus or Locked Jaw.

	1817.	1818.	1819.	Totals.
Charleston,	25	20	14	59
Philadelphia,	9	3	3	15
New York,	3	5	4	12
Baltimore,	2	3	2	7
Boston,	1	0	1	2
	15	11	10	36
Excess in Charleston, above the whole number in the four Cities, }	10	9	4	23

LONDON BILL OF MORTALITY.

Christenings and Burials.

A general bill of all the Christenings and Burials, from Dec. 15, 1818, to Dec. 14, 1819, according to the report made to the king's most excellent majesty, by the Company of Parish Clerks of London, &c.

Christened in the 97 Parishes, within the Walls, 1,277; Buried, 1,149.

Christened in the 17 Parishes, without the Walls, 5,592—
Buried, 4,143.

Christened in the 23 Out Parishes, in Middlesex and Surrey,
13,256—Buried 9,922.

Christened in the 10 Parishes, in the City and Liberties of
Westminster, 4,175—Buried 4,014.

Christened—Males, 12,574—Females, 11,726—in all, 24,300.

Buried—Males, 9,671—Females, 9,557—in all, 19,228.

WHEREOF HAVE DIED—

Under two years of age, - - - - -	4779
Between two and five, - - - - -	1771
Five and ten, - - - - -	826
Ten and twenty, - - - - -	631
Twenty and thirty, - - - - -	1577
Thirty and forty, - - - - -	1990
Forty and fifty, - - - - -	2095
Fifty and sixty, - - - - -	1918
Sixty and seventy, - - - - -	1600
Seventy and eighty, - - - - -	1230
Eighty and ninety, - - - - -	666
Ninety and a hundred, - - - - -	144
A hundred and three, - - - - -	1
Total,	<hr/> 19,228 <hr/>
Decrease in the burials this year, - - -	477

MEDICAL COMMENCEMENT. UNIVERSITY OF PENNSYLVANIA.

At a Public Commencement, held on the 6th April, 1820, the following gentlemen received the degree of Doctor in Medicine, viz.

MASSACHUSETTS.

Names.	Subject of Thesis.
Dudley Atkins,	On Strangulated Hernia.
Fran. D. Wait,	Cholera Morbus.

NEW JERSEY.

Charles F. Matlack,	Tetanus.
Jonathan D. Price,	Abstinence.
George Green,	Muriate of Ammonia and Ferri.
Jeremiah S. English,	Scutellaria Lateriflora.
Marmaduke Burrough,	Uterine Hæmorrhage.

PENNSYLVANIA.

René La Roche,	Inflamed Testis.
David C. Skerrett,	Mod. Operandi of Mercury.
John S. Given,	Rheumatism.
Charles F. Wilstach,	Rest, as a remedy.
Robert E. Griffith,	Stomach and its functions.
Benjamin J. Pennock,	Hepatitis.
Isaac Thomas,	Phlegmasia Dolens.
Samuel G. Morton,	Popliteal Aneurism.
John L. Atlee,	Hydroceph. acutus.
Stewart Kennedy,	Dysentery.
Robert J. Clarke,	Iritis.
John Paxton,	Epilepsy.
Isaac Hays,	Sympathy.
David N. Mahon,	Venesection in Cancer.
Richard Dutton,	Tetanus.
John Elliott,	Ol. Terebinthinæ.
John G. Whilldin,	Dropsy.
John F. Lambe,	Rheumatism.
Jacob S. Rose,	Cynanche Trach.
Samuel P. Reese,	Ol. Terebinthinæ.
Isaac Davis,	Prussic Acid.
Job G. Oslere,	Ergot.
John R. Grigg,	Intermittent.

DELAWARE.

Names.	Subject of Thesis.
Thomas Shivers, jun.	Tetanus.
John M'Callmont,	Dysentery.

MARYLAND.

Francis M. Goldsborough,	Angina Pectoris.
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VIRGINIA.

Robert P. Richardson,	Gonorrhœa.
Wm. B. Scott,	Cynanche Trach.
Le Roy Branch,	Tetanus.
Samuel W. Washington,	Diarrhœa.
George A. Spiller,	Hepatitis.
Amer. V. Payne,	Cholera Infantum.
Christ. B. Fleet,	Hereditary Predisposition.
Thomas J. Goode,	Puerperal Fever.
Alfred Eldridge,	Monarda Punctata.
Robert W. Withers,	Gastritis.
Luke White,	Cholera Infantum.
James May,	Hæmoptysis.
G. L. Corbin,	Aralia Spinosa.
Gustavus V. Jones,	Bilious Inflammatory Fever.
Abner Nash,	Anasarca.
James S. Gunnell,	Dysentery.
Wm. H. Finch,	Necrosis.
John T. Semple,	Hydrophobia.
Wm. Dew,	Stricture of Urethra.
Ph. D. Williamson,	Fever.
Turner Shell,	Scutellaria Lateriflora.
Wm. Jones,	Gastritis.
Peter F. Archer,	Liver and its functions.
Otway Bailey,	Rheumatism.
Isaac Williams,	Indigestion.
Wm. H. Worthington,	Tetanus.
John F. Brooke,	Inflammation of Knee Joint.
John C. Richardson,	Dyspepsia.

SOUTH CAROLINA.

Wm. C. Stewart,	Generation.
Andrew Bonner,	Chimaphilla Maculata.
George Cuthbert,	Phlegmasia Dolens.
Robert L. Armstrong,	Cynanche Trach.

Names.

Subject of Thesis.

Richard Anderson,
Ed. D. C. Jenkins,
Daniel D. Graves,

Amenorrhœa.
Cold Bath.
Pinus Sylvestris.

GEORGIA.

Wm. F. Buchanan,
James P. Screven,
Thomas Hamilton,
Samuel C. Oliver,

Acute Rheumatism.
Hepatitis.
Cholera Infantum.
Progress of Medical Science.

KENTUCKY.

Wm. Clarke,
John C. Boyd,

Peritonitis.
Typhus.

TENNESSEE.

Wm. C. White,
Alexander M'Call,
Wm. M. Watkins,

Pneumonia.
Disease from Poisonous Milk.
Structure and functions of the skin

OHIO.

Thomas Flanner,

Secondary Hæmorrhage.

MISSOURI.

Joseph Brown,

Intermittent.

JOHN REDMAN COXE,
Dean of the Medical Faculty.

Spontaneous Combustion.

[From the Baltimore Chronicle.]

At my mills there was an iron kettle, used for holding ashes—it had remained with ashes from the 5th mo. to the 9th mo., at which time flaxseed oil was, by accident, spilled into the ashes; in about twenty-four hours the ashes were found to be on fire, and wishing to have it fully ascertained, whether it was the oil which occasioned the ashes to take fire, I filled a kettle with cold dry ashes, in which I poured a pint of flaxseed oil, and in twenty-fours I examined it, and found that, as far as the oil had penetrated, the ashes were in a state of combustion; and, on applying some shavings and chips of wood, it immediately caused them to blaze.

From an apprehension that many buildings have been consumed by fires from the foregoing cause, I have been induced to give publicity to the fact.

JOSEPH ATKINSON.

Ellicott's Patapsco Mills, 1 mo. 22d, 1820.

Royal Society, November 11th.

The conclusion of a paper was read, detailing the results of some further researches of Sir Everard Home on the properties of the blood. He has, he considers, discovered that there exist in that fluid globules which are smaller and of a different nature from those it is commonly supposed to contain. They were first observed by Mr. Bauer, whilst examining the layers composing an aneurismal tumour. They were seen in the coat in contact with the circulating blood, in the proportion of one to four, compared with the larger globules; but in the other layers they were more numerous, and in that which have been first formed they existed in the proportion of four to one. Mr. Bauer estimates their size $\frac{1}{2800}$ of an inch. Crystals of muriate and phosphate of soda, and phosphate of lime, were found in making a section of another aneurismal tumour. Sir Everard Home considers that those globules existed originally in the serum; the globules being to be seen only after the blood has coagulated. In coagulated lymph, formed during intense inflammation, these globules were found mixed with a few colourless blood-globules. They were also found in great numbers in the upper firm coat of the buff of the blood, while the lower and softer parts consisted chiefly of red blood-globules. He proposes to call the new globules by the name of globules of *lymph*, to distinguish them from the red blood-globules. Sir Everard Home has also found, that the quantity of carbonic acid gas evolved from buffy blood under an exhausted receiver, is much less than that from healthy blood, and that by far the greatest quantity of this gas was yielded by blood drawn from a healthy person an hour after a full meal. Both lymph globules and blood globules were found in the mucus of the pylorus and duodenum. In chyle, the size of the globules is various.—*Med. and Phys. Jour.*, April, 1820.

Observations respecting the Pathology of Hooping-Cough.

We usually comprise in the *Report of Diseases*, such of the chief results of our practical observations of the current period as are of general application; but we are now induced to give,

in a distinct manner, an account of some which have just occurred to us on opening the body of a subject of whooping-cough, from their seeming to possess a remarkable degree of interest.

A boy, three years old, had suffered whooping-cough about a month, not very severe in degree, and unattended with much difficulty of respiration, excepting for a short time after each fit of coughing. He was a strong, well made child, and had previously enjoyed good health. At the time above indicated, he became affected with great difficulty of respiration, and very severe cough, attended with extraordinarily loud whooping, and long-continued inspiratory efforts. The pulse varied during the two or three ensuing days from 120 to 150; the tongue was clean. There was a moderate degree of expectoration of mucous matter. Leeches, tartar emetic, calomel, and a blister on the chest, had been employed by the attending medical practitioner. On the fifth day after the occurrence of the latter symptoms, in the evening, we first saw the patient. But little hopes of his survival were entertained. The pulse was 170, moderately strong; the countenance somewhat tumid and livid; the breathing extremely laborious, and attended with a rattling sound similar to that heard in croup. The nostrils were widely dilated during inspiration. The fits of coughing were said to be very severe, and attended with intense and long-continued whooping; and the mother said, it appeared as if the air was prevented being expelled from the lungs by some obstacle, the dilatation of them was of such long duration. He died in the ensuing morning. The following were the observations made on dissection, that seem to be peculiarly interesting.

The posterior surface of the epiglottis was of a palish bright-red colour, especially towards its base; the right superior ligament of the glottis was about four times the usual thickness, apparently from inflammation, and its tunic was of a bright-red colour; the left superior ligament was much smaller, apparently only about two or three times the usual size; they both adhered to the lower ligaments, so as to efface the openings into the ventricles of the larynx by false membranes. The

ventricles themselves were full of serous fluid. The lower ligaments were also much thickened, and their mucous tunic of a florid hue. Below this, the whole of the larynx was apparently healthy. At about the third or fourth circle of the trachea, the mucous membrane of this organ began to assume a florid hue, and there was extravasation of blood in it, but no coagulated lymph present. These appearances increased in intensity as we proceeded downwards, and the two lower lobules of the left, and the lower lobule of the right lung, were in a state of extreme sanguineous congestion. The enveloping pleura was of nearly the natural hue, and there was no preternatural effusion of fluid in the chest. The mucous membrane of the bronchiæ, corresponding to these portions of the lungs, was of a very deep-red colour, and much thickened from the increased vascularity, but there was no appearance of effused coagulable lymph. Every thing indicated that the affection of the trachea and lungs was of recent date; whilst that of the parts about the glottis was evidently more chronic, and wholly distinct from the former.

It would be wrong to generalize the application of these observations; but we hope they will induce our readers to examine very particularly the state of the upper part of the larynx in the subjects of hooping-cough; and we should feel much gratified, if any of them will favour us with the results of their observations.—*Ib.*

Prize Question.

The following is proposed by the Society of Sciences of Harlem. The essays should be sent to the Secretary before the 1st of January, 1821.

“What advantages has medicine derived from the reformation and extension of chemistry since the time of Lavoisier, in making us better acquainted with the chemical agency of the medicines usually employed for the cure of several diseases of the human body; and what means should be taken in order to acquire a solid knowledge, useful in medicine, of the hitherto-unknown chemical agency of several medicines.” *Ib.*

Dr. J. W. W. W. W.
Newbury

THE
ECLECTIC REPERTORY
AND
ANALYTICAL REVIEW.

VOL. X.

JULY, 1820.

No. III.

SELECTED PAPERS.

A Statement of Facts tending to establish an estimate of the true value and present state of Vaccination. By SIR GILBERT BLANE, Bart. M. D. F. R. S. Physician in Ordinary to the Prince Regent.

[From the Medico-Chirurgical Transactions, Volume X.]

IT is now twenty-one years since Vaccination was promulgated in this country by Dr. Jenner, and fifteen years since it began to produce a sensible effect in diminishing the mortality from small pox. In regard to the latter period, it is coeval with this society; yet, though no discovery in nature nor in medicine has been more important to the interests of humanity, nor any which ever so rapidly and universally has won the assent and practical adoption of mankind, there are no notices of it on our records, except some allusions to its influence on other disorders, in our second and third volumes. As it is to be hoped that our labours will prove to posterity some of the principal sources of reference regarding the medical and chirurgical discoveries and improvements of the age; as it is one of the reproaches of the country that it has not availed itself so much as any other of the benefits of vaccination; and as there are writers among us who still allege that the failures are so numerous that the value of the discovery is very ambiguous,

it seems one of the duties of the society to lend its aid in placing these important points in their true light.

It seems almost needless to premise, that the small pox is of all maladies that, which, during the last thousand years, has destroyed the largest portion of the human species, and been productive of the largest share of human misery. There is, perhaps, no disease over which medical art has less power; and this power, such as it is, has consisted more in abolishing pernicious practices than in ascertaining any positive methods of controlling its fatality; unless we except the inoculation of it with its own *virus*. But, though the beneficial effect of this on those on whom it is actually practised is undeniable, it has no tendency like vaccination to extirpate the disease; and from the impossibility of rendering it universal, it has actually been found to add to the general mortality of small pox, by opening a new source of diffusion to its *virus*.

In order to bring this to the test of calculation, in order also to institute a comparison of the mortality of small pox as influenced by vaccination, as well as by inoculation from itself, I have selected from the bills of mortality four periods, each of fifteen years, for the purpose of exhibiting the mortality of small pox in each of these series in regard to each other. These are thrown into the form of tables, and annexed to this article.

The first series is the fifteen years immediately preceding the introduction of inoculation, that is, from 1706 to 1720, both included. Previous to this period, no account that could be depended upon regarding the small pox, could be derived from the bills of mortality; for down to the beginning of last century such was their imperfect construction, that small pox, measles, and flux, were blended under one head. Exception may be taken against the accuracy of these bills, even in this improved state, particularly with regard to the discrimination of diseases. This objection, however, is certainly less applicable to small pox than any other disorder, its character being so striking as not to be mistaken by the most ignorant and careless observer.

The second series is taken at the middle of the last century, when inoculation had made considerable progress, that is,

from 1745 to 1759, both included. In comparing this with the preceding series, with regard to absolute numbers, it ought to be taken into account, that eleven parishes were added to the bills of mortality. between the years 1726 and 1745, both included: so that the progressive improvement of general salubrity ought to be estimated still higher than what is indicated by the diminished mortality, as it stands in the tables.

The third series comprises the fifteen years previous to the introduction of vaccination, when inoculation had made still greater progress; that is, from 1785 to 1798, both included.

The fourth series comprises the time in which the vaccine inoculation has been so far diffused as to produce a notable effect on the mortality of small pox; that is, from 1804 to 1818, both included.

The result of these computations stands as follows:

Ratio of the Mortality of Small Pox to the total Mortality.

From 1706 to 1720, one in 12.7; that is, 78 in 1000.

From 1745 to 1759, one in 11.2; that is, 89 in 1000.

From 1785 to 1798, one in 10.6; that is, 94 in 1000.

From 1805 to 1818, one in 18.9; that is, 53 in 1000.

In all these computations, fractions are not noticed in the last column of numbers.

It appears from this statement, that the proportion of deaths from small pox to the total mortality, increased in the course of last century; so that inoculation appears to have added to the mortality. It is but fair to mention, however, that this total mortality is not quite a just scale whereby to measure the relative mortality of small pox; for in the course of that century, the general mortality itself was greatly diminished in relation to the population. This diminution of general mortality was chiefly owing to the diminished mortality of children under two years of age, which, at the time when the account began to be kept, 1729, averaged about 9000; but at the end of the century not more than 5000; also to the decrease of fevers, and still more of fluxes. The relation of the mortality of small pox to the population, would therefore be a more fair criterion of its increase or decrease. In this

view it might, at first sight, be thought that it had decreased; for the population of the metropolis nearly doubled in the course of the last century. But it is to be remarked, that there has been little increase of population in that portion of the metropolis which is included in the bills of mortality; the great increase having been in the parishes of Mary-le-bone and St. Pancras, which are not included in these bills. It is computed in the remarks subjoined to the last parliamentary returns of population, that the population of London, within the walls, had decreased more than three-fifths in the course of last century, from the widening of streets, the erection of public buildings and warehouses; and, it might have been added, from the migration of mercantile families to the west end of the town. As a set-off to this, there has certainly been a great addition, in the same time, to those parishes within the bills, which stand on the verge of the metropolis, such as St. George's, Hanover Square, St. George's, Bloomsbury, Poplar and Stepney. But the addition to the population, if any, within the bills of mortality, does not seem to be so considerable as to affect the computation. And if this is admitted, the absolute numbers of the deaths from small pox, estimated in relation to the population, that is, exactly as they stand on the tables, afford a fair comparative statement of the mortality in the last century, and seem to prove, that inoculation has not added so much to it as has been alleged.*

But the truly important result from these statements consists in the clear, undeniable, and great diminution of it since the introduction of vaccination. It appears, that in the last fifteen years, the mortality from small pox, in the bills of mortality, has not been much more than one-half of what it was in the two like series of years in the middle and latter end of the last century. Nor does this comprise the whole benefit derived from this discovery in the metropolis; for, besides

* It was in the rural population that the effect of inoculation in diffusing small pox was chiefly felt. In this situation there is much less intercourse of persons with each other than in towns, so that not only many individuals escaped from exposure to this infection during their whole life, but whole districts were known to have been exempt from it for a long series of years, before it was universally diffused by inoculation.

that the sixth part of it lies without the bills, it was found, in levying the tax on burials for the last six months of 1794, that the number of unregistered deaths, chiefly those of dissenters, amounted in that half year, to 3148; and the reporter of the parliamentary enumeration thinks that, as besides these there were undiscovered interments, the unregistered deaths may be computed at one-third of the total mortality, that is, about 7000. See *Abstract of the Parish Registers*, 1811, printed by authority of Parliament, page 200.

Assuming, therefore, that vaccination had not been practised the last fifteen years, and that the mortality, from small pox, within the bills, had in that time, that is, from 1804 to 1818, been the same as from 1784 to 1798, that is, 27,569 in place of 14,716; and assuming that there has been the same proportional diminution of deaths in the districts without the bills, and among the unregistered subjects, the account of lives saved in this metropolis by vaccination in these fifteen years, will stand as follows:—

Within the bills of mortality,	12,853
Without the bills of mortality,	2,570
Unregistered cases,	7,711
	<hr/>
Total,	23,134
	<hr/>

The first of these numbers is found by subtracting the amount of deaths by small pox, in the bills of mortality, during the practice of vaccination, from the amount of them, during the same number of years, immediately before the discovery of vaccination.

The second number is found by dividing the first by five. The population of the metropolis without the bills is stated at one-sixth of the whole, which is evidently one-fifth of that within the bills.

The third number is found by dividing the sum of the two others by two. The unregistered cases being as before stated, one-third of the whole.

It appears, therefore, that, even under the very imperfect practice of vaccination which has taken place in this metropolis, 23,134 lives have been saved, in the last fifteen years.

according to the best computation that the *data* afford. It will be seen by an inspection of the table, that, in that time there have been great fluctuations in the number of deaths. This has been owing partly to the small pox inoculation of out patients having, by an unaccountable infatuation, been kept up at the small pox hospital for several years after the virtue of vaccination had been fully confirmed. The great number of deaths in 1805 may chiefly be referred to this cause. Since the suppression of this practice, the adoption of vaccination, though in a degree so incomplete, in consequence of public prejudice, created by mischievous publications, has been unable to prevent a considerable, though fluctuating, mortality from small pox. This mortality, though little more than one half of what it was in former times, might have been entirely saved, if vaccination had been carried to the same extent as in many cities and whole districts on the continent of Europe, in Peru* and Ceylon.

It is now matter of irrefragable historical evidence, that vaccination possesses powers adequate to the great end proposed by its meritorious discoverer, in his first promulgation of it in 1798, namely, the total extirpation of small pox. The first proof of this was at Vienna, where, in 1804, no cases occurred, except two strangers who came into the city with the disease upon them. In 1805 there did not occur a single death from it in Copenhagen.† Dr. Sacco, the indefatigable superintendent of vaccination in Lombardy, stated, in his Annual Report, 3d January, 1808, that the small pox had entirely disappeared in all the large towns in that country; and that in the great city of Milan it had not appeared for several years.

* In the summer of 1811, the author was called to visit, professionally, Don Francisco de Salazar, who had arrived a few days before in London, on his route from Lima to Cadiz, as a deputy to the Spanish Cortes. He informed me, that vaccination had been practised with so much energy and success in Lima, that, for the last twelve months, there had occurred, not only no death from, but no case of, small pox; that the new-born children of all ranks are carried as regularly to the vaccinating house, as to the font of baptism; that the small pox is entirely extinguished all over Peru; nearly so in Chili; and that there has been no compulsory interference on the part of the government to promote vaccination.

† See Pfaff *neun nord v. Archiv. B. I.*

Dr. Odier, of Geneva, so favourably known for his high professional, scientific, and literary acquirements, testifies, that after a vigorous perseverance in vaccination for six years, the small pox had disappeared in that city and the whole surrounding district; and that, when casually introduced by strangers, it did not spread, the inhabitants not being *susceptible*. The Central Committee in Paris testify, in their report of 1809, that the small pox had been extinguished at Lyons and in other districts of France.

These are selected as some of the earliest proofs of the extirpating power. But it is demonstrable, that if, at the first moment of this singular discovery, at any moment since, at the present or any future moment, mankind were sufficiently wise and decided to vaccinate the whole of the human species who have not gone through the small pox, this most loathsome and afflicting of all the scourges of humanity would instantaneously, and for ever, be banished from the earth.

It is farther manifest, that extirpation being the sole and ultimate aim of this discovery, and there being the fullest historical and practical evidence of its being capable of accomplishing this end, all other questions with regard to its expediency must be futile and irrelevant. It is in the nature of all morbid phenomena, to be liable to exception. One of the most essential and characteristic laws of small pox itself, namely, that of its affecting the human subject but once in life, is found, in rare cases, to be violated. It is, therefore, perfectly conformable to analogy, and naturally to be expected, that it may not in all cases be a complete security against small pox. But it is obvious, that, admitting these exceptions to be very frequent, much more so than the recurrence of small pox after small pox, this can constitute no objection to the practice, as long as the extirpating power remains unimpaired and unimpeached. Nay, it is obviously so far from an objection, that it ought to operate as a powerful additional incentive on every benevolent mind, to push vaccination to the utmost, as rapidly as possible; in order that those who are still susceptible, either from peculiar natural constitution, or from the unskilful manner of conducting the operation, or from defective matter, may not, by any possibility, catch it; for, in the event

of its extirpation, it could no where be met with. And in order to stimulate the good and the wise to aim strenuously at this consummation, let it be constantly borne in mind, that the adversary they are contending with, is the greatest scourge that has ever afflicted humanity. That it is so, all history, civil and medical, proclaims: for, though the term plague carries a sound of greater horror and dismay, we should probably be within the truth, if we were to assert, that small pox has destroyed a hundred for every one that has perished by the plague.

It is true, that in its last visitation of this metropolis, one hundred and fifty-four years ago, it carried off 70,000 victims in a few months; but since that time, the deaths from small pox, recorded in the bills of mortality, have amounted to more than 300,000; and a like number of the survivors have been afflicted with blindness,* deformity, scrofula, or broken constitutions, which is not the case with the plague. And when it is considered that there are large portions of the globe, India, China, even one whole quarter of it, North and South America, besides all the tropical and arctic regions, in which it has never been known; and that in all the countries liable to it, it seldom appears but at one season of the year, and sometimes at long intervals, the ravage which it makes is trifling when compared with the unceasing havoc of small pox, which spares no nation in any climate, or at any season.

The preceding reasoning is grounded on the supposition of extirpation; but, however demonstrable the *possibility* of extirpation may be, it may not in all communities be *practicable*; and may not these alleged failures so operate, as, in such cir-

* It appears by a report of the hospital for the indigent blind, that two-thirds of those who apply for relief have lost their sight by the small pox. It is alleged by some of the soundest political economists, that small pox does not diminish the numbers of mankind, nor vaccination increase them; for population is determined by subsistence, and the indefinite powers of procreation soon repair the ravages of disease. But, however this may be, the miseries incident to so many of those who survive small pox, whereby they become a burden to themselves and to society, and this disease uncontrovertibly an evil of the first magnitude, not to speak of the intense sufferings and afflictions inseparable from it.

cumstances, to render the expediency of the practice questionable?

In order to decide this, let the nature and amount of these failures be ascertained and estimated.

The description of those cases of small pox, if they can be called so, which occur in vaccinated subjects, is shortly as follows. The invasion and eruption in every respect resembles that of the genuine small pox. I have seen it attended with high fever and a thick crowded crop of *papulæ*, such as precedes the most severe and dangerous cases of the confluent kind. This runs on till the fifth day from the eruption, both days included, at which time some of the *papulæ* begin to be converted into small sized pustules. The disorder then abruptly stops short. On the following day the fever is found to have subsided, with a shrivelling and desiccation of the eruption, and recovery proceeds without the least danger or inconvenience. The face is marked, for some time after, with brown spots, but without pits. It should never be forgotten, that all morbid *phenomena* are full of varieties and exceptions. Accordingly, though the fifth day is the most common limit of this disorder, it sometimes stops short on the third; sometimes not till the sixth or seventh; and in a very few cases it has been known to run the common course of small pox. What forms the strong line of distinction from proper small pox, is that, with a few exceptions, it does not proceed to maturation and secondary fever, which is the only period of danger. I am not prepared to deny that death may not have occurred in a few instances, nay, there seems sufficient evidence that it actually has; but these adverse cases are so rare, as not to form the shadow of an objection to the expediency of the general practice. A few weeks ago, at a meeting of this Society, at which forty members and visitors were present, I put the question whether any of these eminent and extensive practitioners had met with any fatal cases of this kind. Two gentlemen had each seen a single case, and two other gentlemen took occasion to say that they had each seen a case of second small pox, both of which proved fatal. It is evident, therefore, that according to that maxim which guides mankind in the conduct of life, namely, that of acting on a general rule and ave-

rage, and not on exceptions, these adverse instances ought not to have the least influence on practice, even though they were much more numerous.

As it is of the utmost consequence to establish the strong and important distinction between small pox, properly so called, and that which takes place after vaccination, which may be called the mitigated, or five-day small pox, a few of the most impressive testimonies respecting the safe nature of the latter may be here recited. Mr. Brown,* of Musselburgh, gives the detail of forty-eight cases, in none of which did the secondary fever or death occur. Here was a saving of at least eight lives, at the lowest computation, for this is the number which by the average mortality of natural small pox would have died, if the constitutions of these forty-eight persons had not been modified by previous vaccination. Dr. Dewar, of Edinburgh, hearing that many vaccinated subjects had been affected with small pox at Cupar in Fife, where the natural small pox at the same time prevailed, he most laudably repaired to the spot to investigate the subject. He found that fifty-four vaccinated subjects had caught the small pox. All these, except one, had the mitigated or five-day eruptive fever and lived. The fatal case, was that of a child, who had a complication of other disorders, and having died on the fifth day, the small pox, according to its ordinary course of fatality, could not of itself be the cause of death. All the rest were safe; while of sixteen cases of the natural small pox at the same time and place, six died; so that if these fifty-three cases had not undergone the mitigating process of vaccination, nineteen or twenty would have perished. Between thirty and forty cases of the same kind have occurred at Carlisle, on the testimony of Dr. Barnes, a respectable practitioner of that city.† Many proofs might be adduced from the oral testimony

* See Inquiry into the Antivariolous power of Vaccination. Ed. 1809. There is an article in the Edinburgh Medical Journal by the same gentleman in 1819, in which he mentions that he had heard of several deaths having occurred from cases of small pox after vaccination. But, admitting this, it is utterly incomprehensible, by what process of reasoning Mr. Brown could, on such premises, arrive at the conclusion, that vaccination ought to be exploded and abandoned.

† See a clear and able exposition of this subject in the Medical and Surgical Journal of Edinburgh for July, 1818, by Mr. Dunning of Plymouth.

of private practitioners, which would over swell this article. The only other to be mentioned is from the report of the Central Committee of Vaccination at Paris, made in December last, in which the description of the disease occurring after vaccination corresponds exactly with the mitigated five-day cases which have occurred in Britain. They refuse the name of small pox to it; but as I know from my own observation, as well as from the testimony of others, that the matter from it does by inoculation give the small pox, we can hardly perhaps with propriety deny it that name; but it should be distinguished by some strong discriminating epithet, such as is suggested above.

Now let all this be applied to the case of a community, in which the total eradication of small pox is quite hopeless. Let it be admitted that such occurrences as have been described do frequently occur: let it even be admitted, for argument's sake, that every vaccinated case whatever must of necessity and unavoidably, at some time or other in future life, be affected with this mitigated species of small pox, would it not even under this great abatement be one of the greatest boons that could be conferred on humanity, as an instrument or remedy which would disarm small pox of its danger? The next greatest benefit to the total extirpation of small pox, would be the stripping it of its terrors by rendering it safe and harmless.

It may be further remarked, that the benefit derivable from the different proportions of the persons vaccinated to the total population, advances in a considerably higher progression than the simple arithmetical. It is evident that the smaller the relative number of the vaccinated, the greater their chance of meeting with small pox infection; and that though the disease they may catch is of a mitigated nature, it would nevertheless be desirable to avoid it on its own account, but still more on account of the prejudice it creates. This, in the eye of general benevolence, constitutes an additional, though secondary motive, for extending the vaccine inoculation as widely as possible, even though the attainment of the *maximum* of total extirpation should be impracticable and hopeless.

It is of the highest importance to society that this subject

should be seen in its true light, and in all its bearings; for the frequent occurrence of these cases of small pox, however safe in themselves, have had a most pernicious effect on the credulous and ignorant, by giving a check to the practice of vaccination. How many parents are there now who, from a weak distrust in the virtue of vaccination, have to lament the loss of a child from small pox, either casual or inoculated? Many such are known to myself. It is pleasing, however, to observe, that though this unmerited discredit into which vaccination had fallen, swelled the number of deaths in London from small pox to 1051 in 1817, good sense is likely still to prevail; for last year (1818) the deaths have fallen lower than they have ever been known since the institution of the bills of mortality, the total number being only 421.

On the whole matter, I believe I am speaking the language of every man of good principles and feelings, capable of reflecting seriously and considerately on the subject, when I say, that whenever he applies his mind to it, he finds some new and increasing cause of complacency and satisfaction. Viewed as a mere physical fact in the natural history of the animal kingdom, the virtue of the vaccine *virus* in resisting the action of the *variolous*, is, by its novelty and singularity, highly striking and interesting to every one whose taste leads him to take delight in contemplating and exploring the devious ways and varied forms of nature, as curious exceptions to the uniformity and constancy of her laws. But the importance of this vanishes to nothing when the unexampled benefits of it to mankind are fairly weighed; benefits which could never have been dreamt of by the most sanguine philanthropist, who, in contemplating it, finds himself lost in astonishment, at a boon to mankind almost beyond the grasp of his mind duly to appreciate. It will in the eyes of future ages be deemed an *epocha* in the destinies of the world, and one of the highest boasts of the country in which it took its rise, with a sense of unrequitable obligation to the individual who first disclosed and promulgated the secret, by drawing it from the dark recesses of rural tradition, and rendering it available to the whole human race.

Such are the sentiments which must fill every well consti-

tuted mind, and it behoves the whole medical profession, which has already done itself so much honour by the zealous and disinterested encouragement afforded to it, to continue its efforts in eradicating every remaining prejudice against it. It becomes Englishmen, in particular, to foster it, not only as the native offspring of his country, of which he has reason to be proud, but to redeem the character of the nation from the reproach of having of all others, whether savage or civilized, done the least justice to this noble discovery. There is no country which has prized it less, nor availed itself of it so little. Have we not seen it adopted instantly in Peru, in consequence of a flash of conviction from the light of evidence, and have we not seen this conviction fully justified by the immediate disappearance of small pox from that whole region? To those nations who may feel an envy of the glory attached to our country by this discovery, it must be no small consolation to perceive, that a large proportion of the English nation has hitherto been so besotted as not to know how to appreciate nor to avail itself of it, and that it has encountered more opposition among ourselves than in all the world besides.

TABLE I.

TABLE I.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1706	22,097	1095	1 in 20	50
1707	21,600	1078	1 20	50
1708	21,291	1687	1 12 $\frac{1}{2}$	79
1709	21,800	1024	1 21	49
1710	24,620	3138	1 8	127
1711	19,833	915	1 21 $\frac{1}{2}$	46
1712	21,198	1943	1 11	92
1713	21,057	1614	1 13	77
1714	26,569	2810	1 9 $\frac{1}{2}$	106
1715	22,232	1057	1 21	47
1716	24,436	2427	1 10	100
1717	23,446	2211	1 10 $\frac{1}{2}$	94
1718	26,523	1884	1 14	71
1719	28,347	3229	1 8 $\frac{3}{4}$	114
1720	25,454	1440	1 17 $\frac{1}{2}$	56
Total,	350,503	27,552	1 12.7	78

In this series it appears that the deaths from small pox are, to the total mortality, as 1 in 12.7; that is, 78 in 1000.

TABLE II.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1745	21,296	1206	1 in 17 $\frac{3}{4}$	56
1746	28,157	3236	1 8 $\frac{3}{4}$	115
1747	25,494	1380	1 18 $\frac{1}{2}$	54
1748	23,869	1789	1 13 $\frac{1}{2}$	75
1749	25,516	2625	1 9 $\frac{3}{4}$	103
1750	23,727	1229	1 19 $\frac{1}{4}$	52
1751	21,028	998	1 21	48
1752	20,485	3538	1 5 $\frac{3}{4}$	172
1753	19,276	774	1 25	40
1754	22,696	2359	1 9 $\frac{1}{2}$	104
1755	21,917	1988	1 11	91
1756	20,872	1608	1 13	77
1757	21,313	3296	1 6 $\frac{1}{2}$	155
1758	17,576	1273	1 13 $\frac{3}{4}$	73
1759	19,604	2596	1 7 $\frac{1}{2}$	132
Total,	332,826	29,895	1 11.2	89

In this series it appears that the proportion of deaths from small pox is, to the total mortality, as 1 in 11.2; that is, 89 in 1000.

TABLE III.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1784	20,454	1210	1 in 17	59
1785	18,919	1999	1 9 $\frac{1}{2}$	106
1786	20,445	1210	1 17	59
1787	19,349	2418	1 8	125
1788	19,697	1101	1 17 $\frac{3}{4}$	56
1789	20,749	2077	1 10	100
1790	18,038	1617	1 11 $\frac{1}{4}$	89
1791	18,760	1747	1 10 $\frac{3}{4}$	93
1792	20,313	1568	1 13	77
1793	21,749	2382	1 9	11
1794	19,241	1913	1 10	99
1795	21,179	1040	1 20 $\frac{1}{4}$	49
1796	19,288	3548	1 54	18
1797	17,014	512	1 33 $\frac{1}{2}$	30
1798	18,155	2237	1 8	123
Total,	293,850	26,579	1 11	90.9

In this series it appears that the proportion of deaths from small pox to the total mortality is 1 in 11, that is, 90.9 in 1000.

TABLE IV.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1804	17,038	622	1 in 27 $\frac{1}{2}$	36
1805	17,565	1685	1 10 $\frac{1}{2}$	96
1806	18,334	1297	1 14	71
1807	17,938	1158	1 15 $\frac{1}{2}$	65
1808	19,964	1169	1 17 $\frac{1}{4}$	58
1809	16,680	1163	1 14 $\frac{1}{4}$	70
1810	19,893	1198	1 16 $\frac{1}{2}$	66
1811	17,043	751	1 22 $\frac{3}{4}$	44
1812	18,295	1287	1 14 $\frac{1}{4}$	70
1813	17,322	898	1 19 $\frac{1}{4}$	52
1814	19,783	638	1 31	32
1815	19,560	725	1 27	37
1816	20,316	653	1 31 $\frac{1}{4}$	32
1817	19,968	1051	1 19	53
1818	19,705	421	1 47	21
Total,	279,404	14,716	1 18.9	53

In this series it appears that the proportion of deaths from small pox to the total mortality is 1 in 18.9, that is, 53 in 1000.

An Account of some Alarming Symptoms caused by Spiculæ of Bone in the Stomach. By JOSEPH M'SWEENEY, M. D.

[From the Edinburgh Medical and Surgical Journal for January, 1820.]

ALTHOUGH the power of digestion has been found capable of subduing very hard substances, yet the human stomach, which often with difficulty digests solid articles of food, may be found sometimes incapable of acting on so solid a substance as bone. Even where the power of the organ is capable of acting on such a substance, in some cases so long a time may be necessary, that the foreign body may be productive of serious consequences. It is not to be inferred, that every substance difficult of digestion, or indigestible, is kept from passing out of the stomach. Daily experience shows, that the pylorus does not keep so strict a guard. Different indigestible substances have passed through the alimentary canal; even articles of food have passed unchanged. No doubt, pieces of bone of an unirritating shape have also passed without causing inconvenience. The pylorus not being irritated by a smooth body, can gradually dilate, and can allow it to pass without producing uneasiness; but where sharp spiculæ of bone have been swallowed unnoticed, alarming, perhaps fatal consequences have ensued, without the cause being suspected. From the consideration of the pylorus, we must perceive the obstacles it will oppose to the passage of a sharp body. It is endowed with contractile power, and is sensible to a stimulus. The contact of a sharp body must stimulate it violently, and must consequently cause it to contract, and thus oppose the passage of that which irritates it.

A gentleman at dinner swallowed a small fragment of bone. On the same evening, uneasy symptoms came on. He felt a pain in the centre of the scrobiculus cordis rather dull, but causing great anxiety. It sometimes changed its situation, but was mostly referred to the centre of the scrobiculus cordis. He sometimes had intervals of ease, at other times his sufferings became aggravated in a violent degree. He then could not point out the situation of the pain so exactly as before. It was felt more internally, as if some part inside was grasping

some sharp body. In this state his body was bent, his hand was firmly pressed against the region of the stomach, his pulse became small and quick, a cold sweat broke out over the surface, and his anxiety arose to an alarming degree. From this state he generally found relief by change of posture, and the pain mostly resumed its former situation in the centre of the scrobiculus cordis. He had a constant desire of keeping the hand on the situation of the pain. I had recourse to diluted muriatic acid as strong as I could venture to give it. All bad symptoms ceased under its use, and he found no inconvenience from the acid, which freed him from the state of suffering in which he had been. I met with similar symptoms in the case of a gentleman who was conscious of having accidentally swallowed a fragment of bone at dinner, a good many days before. I had recourse to a similar practice, and with complete success.

Mrs. P—le, of Nicolson Street, in Edinburgh, consulted me last winter about a dull kind of pain in the region of the stomach. Not being decided as to the nature of it, I merely recommended attention to regimen. She applied to me a second time, and stated that she was not better, and complained of the great anxiety it caused her. I now suspected, that the pain might arise from a spicula of bone accidentally swallowed. On close attention to the symptoms, I found they were the same as those stated above. She had the constant desire of keeping the hand on the situation of the pain, which was mostly felt in the centre of the scrobiculus cordis. There were intervals of ease; at other times the sensation of a sharp body being grasped, threw her into a state of agony. In this state, the pulse became small and quick, and a cold sweat broke out over the surface. Although she had not the least recollection of having taken any thing that would account for the symptoms, yet I did not hesitate in thinking, that the case was similar to those mentioned, particularly as she had taken food in which a spicula of bone might have been. I added as much acid to a half pint of water as I thought would be sufficient to relieve the symptoms without injuring the stomach. She took this quantity at one draught; but my disappointment was indeed great, when she complained of being rather worse

on the following day. The acid had evidently increased the irritability of the stomach without acting on the foreign body. I now supposed that the acid had passed out of the stomach without remaining a sufficient time to act on the spicula of bone; and as the irritability was increasing daily, I determined not to lose time, but to have recourse to the most vigorous practice at once. I made the diluted acid much stronger than what she had taken the day before; and having taken a quantity of it to be certain that it was not too strong to injure the stomach, I explained her case to her, and she willingly agreed to adopt any plan of treatment I should propose. She took a wine glass of this diluted acid every ten minutes. After a half pint of it was taken in this manner, she took the same quantity in one draught, and I was happy to find, after it, that the pain and anxiety had ceased, with which she had been in agony during thirteen days. She now said she felt only a general sense of soreness; she slept well that night; next day she still felt the general sense of soreness; but she was in good spirits, and completely free from the pain. Having recourse to the remedy quickly on account of the urgency of the symptoms, unfortunately I did not ascertain the strength of the diluted acid I used. The standard I employed was to try it on myself in every case, and to take it as strong as I could with safety. The plan employed to prevent the teeth from being injured, was, to make the patient open the mouth as wide as possible, and to protrude the tongue so as to cover the teeth of the lower jaw; the edge of the glass was then rested on the tongue, and the diluted acid was swallowed without allowing it to come in contact with the teeth of the upper jaw. The mouth was repeatedly washed with water. In this manner, the teeth escaped uninjured even in the case of the patient who had been affected thirteen days, and who took a great deal of the acid.

If this patient had been allowed to remain without medical assistance, it may be fairly presumed, that a fatal gastritis or ulceration of the stomach might have at length ensued. A thickened or ulcerated state of the pylorus might have been caused, and I am afraid, that affections of this kind have been classed under the title of scirrhus pylorus, while the cause of

the mischief has not been suspected. It is possible that an ulceration would take place, through which the contents of the stomach would be extravasated into the abdomen. A small fragment of bone might easily escape observation when enveloped in the ingesta of the stomach. It would be a desirable thing that the attention of examiners would be in future directed in search of such a cause, in cases of extravasation of the contents of the stomach into the abdomen, and in cases of diseased pylorus. There is some danger of confounding the symptoms with those of cramp of the stomach, as a patient may have swallowed a spicula of bone without being aware of it. The bent position of the patient, the contortions of his body, his hand pressed against the seat of the pain, the anxiety depicted on the countenance, and the intervals of ease, may deceive a practitioner suddenly called in, and may lead to injurious practice. These symptoms may be distinguished from those of the cramp of the stomach, by the following marks: The pain is mostly felt in the centre of the scrobiculus cordis, and is described as if it could be covered with the point of the finger. In this situation, although the patient has a desire of pressing the hand gently against the seat of the pain, yet very firm pressure increases it. When the pain is felt more internally, causing the sensation of something being grasped, firm pressure gives relief, the pulse becomes quick and small, and a cold sweat breaks out over the surface. Perhaps it may be asked, would not it be better to employ an emetic in such a case? A person took an emetic on account of having accidentally swallowed a sharp body, and soon after expired. As the surgeon who was called in to examine the body, and by whom I heard the case related, will probably give the particulars in some periodical work, I shall not anticipate him; but the account of the dissection was quite sufficient to prevent me from employing an emetic. As a sharp body is prevented from passing, on account of its irritating the pylorus, a purgative medicine was, I think, contra-indicated; it would be likely to make the pylorus suffer more. The diluted acid must have afforded relief by acting on the sharp irritating points which were fortunately most exposed to its influence, and thus allowing that to pass

which was before prevented by the irritating points causing the pylorus to contract violently, when they came in contact with it.

Cases of Severe Dyspnœa in Children, connected with a Singular Affection of the Throat. By JOHN ABERCROMBIE, M. D.

[From the Edinburgh Medical and Surgical Journal, for April, 1819.]

I HAVE met with three cases of violent difficulty of breathing in children, connected with an affection of the throat which was new to me, and perhaps may be not unworthy of a corner in your journal.

CASE I.—Was a child about two years of age. I found him affected with severe difficulty of breathing, resembling the last stage of croup, inspiration being particularly laborious, and performed with a strong exertion of all the muscles connected with it, and a sound which conveyed the impression of great interruption to the passage of air through the larynx. At first sight I considered the disease as croup; but on enquiring into the history of it, I was told that the affection had existed in nearly the same degree of violence for eight days; and that the health of the child had not been in other respects remarkably impaired. Concluding from these circumstances that the disease could not be croup, I was led to a particular examination of the throat, where I found the tonsils and other superior parts quite healthy; but in the lower and posterior part of the pharynx I observed an appearance of fulness, which, however, was situated too far down to be seen distinctly. On examining this part with the finger, I discovered a tumour, arising from the posterior part of the pharynx, and filling up the space completely, so as to press considerably upon the larynx;—the tumour was soft and fluctuating, and evidently contained a fluid. With considerable difficulty I succeeded in puncturing it, by directing a lancet along the fore finger of the left hand, which was kept in contact with the tumour, and purulent matter was discharged from it, with immediate and complete relief of all the symptoms. In the course of the fol-

lowing night, however, the dyspnœa returned; and on examining the throat next day, I found that the tumour was again filled with matter. It was therefore necessary to puncture it again, by which the dyspnœa was again completely relieved. In this manner it required to be punctured four or five times, after which the case terminated favourably.

CASE II.—Was a child aged eighteen months, who had passed through measles eight or ten days before I saw her. Her breathing had first become difficult during the eruption of measles, and after partial relief, procured by the usual treatment, had again become difficult after the decline of the eruption. The difficulty had been gradually increasing, particularly during the last three days, and in the night before I saw her, had been so great as frequently to threaten immediate suffocation. On examining the throat, I discovered a tumour which exactly resembled that described in the former case, except that it was situated rather lower down, so that it could only be felt by the finger, and nothing remarkable could be detected by looking into the throat. With some difficulty the tumour was punctured by means of a small trocar, such as is used for hydrocele, the canula being first carried down so as to bring the point of it into contact with the tumour, and then the stilette introduced through it. There was a copious discharge of purulent matter, with immediate relief of the symptoms; and I requested an intelligent young medical man, who was related to the family, to make pressure on the tumour several times in the day, so as to prevent it from filling again, as in the former case. By this precaution there was no return of the symptoms. A remarkable circumstance in this case was, that the child appeared to have little or no difficulty in swallowing.

CASE III.—Was so exactly similar to the preceding, that it is unnecessary to describe it particularly. The tumour required to be punctured a second time, after which the case terminated favourably.

Though not immediately connected with that subject, I take the opportunity of adding a very few observations on the larynx as a seat of disease. The more violent diseases of this organ have been investigated with much attention; but I think there are more obscure affections of it, which present a most

important field for observation, in which, hitherto, little has been done. Many obstinate cases of *cough* probably have their seat in the larynx, which we are sometimes disposed to refer to the lungs. I lately saw a gentleman, about fifty years of age, who was affected with the most violent cough I ever met with. It occurred with greatest violence in the night time, when it frequently kept him for an hour or two at a time in one continued paroxysm of the most violent convulsive coughing; his features became turgid; he nearly lost his recollection; and there seemed every reason to fear that the paroxysm would terminate either by suffocation or apoplexy. The disease had continued about a fortnight, and the most active remedies, large and repeated blood-letting, blistering, opiates, and antispasmodics, had not made the slightest impression upon it. His pulse and breathing were natural; he had a peculiar hoarseness or huskiness of his voice; and, upon inquiry, he mentioned that the paroxysms were preceded by a particular sense of irritation about the larynx. The application of six leeches to the region of the larynx broke the force of this frightful disease, and a second application of the same remedy completely removed it.

It may be worthy of investigation, whether whooping-cough has any relation to the larynx. Some cases also, which have been considered as consumptive, have afterwards turned out to be connected with disease in that organ; and, upon the whole, there seems to be much room for extensive observation, with the view of introducing precision and accuracy into the pathology of cough.

Observations and Experiments on the Nature of the Morbid Poison called Yaws. By JAMES THOMSON, M. D., &c.

[From the Edinburgh Medical and Surgical Journal, for July, 1819.]

THE general appearance of the eruption in this disease has been accurately described by every writer on the subject, but many particulars necessary to an accurate knowledge of the

laws that it follows have been entirely omitted, or rather never ascertained. Many British practitioners have exclaimed against this neglect, and the more so, as the disease is of so common occurrence in many of the West India islands, that it comes within every one's observation. To a person who has resided any time in the West Indies, the cause of such neglect is easily explained. There is a sort of actual horror on the countenance of most practitioners at the sight of a patient covered with the eruption. He views him afar off, and his opinion is given in the most hurried manner. The dreadful stories of white people having been infected, the shame attached to it, and, more than all, the fear of his prospects in life being blasted, are sufficient to counteract every desire of advancing his knowledge of the disease. The consequence of this is, that the yaw patients are banished to some remote corner of the estate, where they are put under the care of some old woman, who keeps the sores clean, and conducts them through the disease. If, from neglect, symptoms called the dregs of the yaws should appear, or if any hereditary tendency should have been called into action, the practitioner may again see the patient, but he has only one mode of practice, an alterative course of mercury, and *that* the patient *must* undergo. If the symptoms should cease, in a short time they again return,—a second course must be submitted to, for the first was not sufficient. I am ashamed to mention the number of such courses I have seen a poor creature go through, and I hardly would get credit if I related what has been told me by old practitioners. Such symptoms, I shall afterwards show, are, in general, incurable; all we can promise is to mitigate them with a generous diet, and the use of the woods employed by the natives. One would have thought that their success might have taught medical men to follow their plan; but no, there is a charm in an alterative course that has sent thousands to their graves. Happily for mankind, the charm is fast losing its power, and in a few years practitioners will ridicule our notions of its virtues. A perusal of the excellent production of Dr. Adams on morbid poisons first induced me to turn my attention to this subject, and I trust he will find some of his queries answered in this paper; and nothing could give me greater pleasure than to

satisfy the doubts of a physician so eminently qualified to extend our knowledge by his accurate mode of observation.

This disease is constantly preceded by symptoms of languor and pain, amounting in most cases to fever, which is more severe in children. Before the eruption appears, I have frequently seen the whole surface covered with a white scurf. It looks as if the skin was dusted with flour. It remains only a few days, and then small pimples may be seen on the forehead and other parts of the body. These continue to increase for six or ten days, when they get a crust on the top, and ill formed pus can be detected. The pustules go on increasing in size, covered with loose irregular crusts; many of them acquire the size of a shilling. If you remove this crust, you find a foul sloughy ulcer beneath. I have endeavoured to ascertain the exact period when you can find the fungus to shoot up, but it is irregular. I have found it so early as one month from the eruption, and so late as three. We cannot, therefore, take this as the criterion of the second stage of the disease. It seemed to depend much on the constitution of the patient, and appeared sooner on those that were well fed. A second attack of fever will occasionally bring out a new eruption. These proceed in the same way, so that we have sores in all states on the body at the same time. When the disease attacks the throat, it is extremely distressing; the ulcers resemble a piece of *toasted cheese*; they never have any fungus; and before the susceptibility is destroyed, the patient is deprived of the greater part of the palate.

The yaw shows a difference according to the subject. In healthy patients it is red, like a piece of flesh; when weakly or diseased, it is white like a piece of cauliflower, and bleeds on the slightest touch, and is by no means so elevated. In this state it remains for some time, till the fungus gradually contracts, diminishes in height, and is finally covered with skin. The yaw leaves no mark in general, except in those places where the inflammation has run very high; the scar in that case resembles the cow-pox scar, only it is broader, and more superficial.

The duration of the disease is various. Some will get rid of it in six months, others not till twelve; the average may be

stated at eight. The violence of the eruption and fever does not seem to depend, as in other exanthemata, on the peculiar susceptibility of the patient, but merely on his bodily strength at the commencement of the disease; for in weakly ill fed children it is uniformly more severe. The hairs are represented by many authors to be turned white. This I am confident is erroneous, and proceeds only from want of cleanliness. In all cases where proper ablution is used, no such thing can be observed. Where the hair was destroyed, it seldom ever returned, and then it was of the natural colour. The disease is classed among the tuberculæ in the arrangement of Willan. What right it has to be placed there I cannot imagine; for surely a tubercle and a fungus are quite distinct, and the fungus comes on at a late period of the disease. The eruption is at first papular, then pustular, and after that rises the yaw, but this is not constant; the ulcer may heal without any rising. If, therefore, we must have it to conform to an artificial arrangement, it must be classed among the papulæ, then transferred to the pustulæ, and afterwards carried forward to the tuberculæ; but if no fungus shoots up, it must remain as a pustular disease; or, if we will be still more scientifically minute, those sores that have a fungus must be called tubercular, and those that have not, pustular, even though they are within a few inches of each other. Nosology has already led to more absurd conclusions, and it will still continue to involve those who patronize it in more ridiculous difficulties.

The first question I endeavoured to ascertain was, the *latent period*. Chance first informed me of this. A number of healthy negroes were removed from a mountainous situation to a sugar estate. The children were mixed with those already on the property, and had their meals together. Seven weeks after their intercourse, three were seized with fever and pains; the eruption appeared all over the body; the rest, at the end of ten weeks, showed symptoms of the disease, and in eight months they had all recovered. Not satisfied with this accidental experiment, I was allowed to inoculate a child with matter. I ordered it to be taken from ulcers where the scab had been removed, and inserted in five different places. Three of the punctures healed, the other two did not heal for three

weeks; they resembled a scratch that proves irritable. From that time small sores appeared, which increased until they became foul, sloughy, ragged ulcers. Seven weeks from the time of insertion, pimples appeared on the forehead, and over the body, the fungus shot up, and the patient had a copious eruption, and did not get clear of the disease before nine months; the sores had no particular appearance. I may remark, that the two ulcers on the arm never showed any fungus, and left deep scars.

The small-pox having broke out in a mountainous situation, recourse was had to inoculation. Matter was taken from an apparently healthy negro girl, who had already been inoculated for the small-pox. This matter was inserted into the arm of only one subject. When the negro girl knew this, she came in great anxiety and confessed she had the yaws. It could not be believed, for her skin was shining and quite healthy, but on lifting up the arm, three sores were found in the axilla. The patient that had been inoculated had the small-pox mildly. At the end of two months the yaws appeared, and were neither milder nor shorter in their course than usual. The puncture made where the small-pox was inserted healed up in the proper time, and the secondary pustules were all dried up long before yaws appeared. This case might lead us to conclude, that inoculating with the circulating fluid of a yaws patient will produce the disease; but I have since found, that it is not the case, and that there must have been some undiscovered cause of fallacy in the example of the negro girl. I ordered four children to be inoculated, each in five places, with blood taken from a subject covered with yaws. The punctures healed immediately. In this fact, the yaws follows the course of the facts known with regard to other morbid poisons. The case of yaws mentioned by Dr. Adams as occurring in a Danish nobleman who had been *ten* months absent from the West Indies, is at variance with the facts just stated. I will not venture to reconcile them. I may merely state, that several old practitioners, to whom I have mentioned the fact, were of opinion, that the latent period could never have been so long. These cases also go to prove that inoculation does not render the course of disease either shorter or milder, and many other considerations confirm me in this opinion.

The patient is liable to other exanthemata during the yaws. I have known them to have small-pox and chicken-pox. I ordered several yaws children to be vaccinated. The cow-pox went regularly through its different stages; there was no observable difference in the time of healing in the sores. The matter was not taken in these cases. I did not feel myself warranted in doing so.

Matter was taken from an active yaws sore, and inserted into the axilla of a rabbit and a dog. No effect followed. It was repeated twice, but the punctures always healed. Matter was made up into a bolus, and given to a dog and a fowl, with no effect. There is an eruptive disease in cattle that the negroes call the yaws, but there is no analogy between them.

This disease can only be communicated once; the susceptibility is destroyed; but there frequently happens very unpleasant symptoms to a person who has undergone the eruption. For example, a mother who gives suck to a child with yaws, has the nipple excoriated from the acrid matter around the mouth; the irritation spreads, and a foul ulcer consumes the breast, and produces death by spreading to the axilla; the child also perishes, unless supported by the greatest care, for it is abandoned by every person.

There is a singular fact with regard to the history of the yaws not generally known but not the less certain. If a person with a large ulcer contract the yaws, he may pass through the disease without any eruption. At a certain time, the sore begins to acquire a new granulated appearance, and becomes elevated, the edges of the ulcer become characteristic, and a constant discharge is kept up. If this sore is attempted to be healed, and we succeed, an eruption of yaws follows in the regular manner. Otherwise, it goes on, and generally remains as an ulcer for life. The practice always to be pursued is, to endeavour to heal the sore the moment we suspect any infection.

I have endeavoured, by every means, to ascertain whether the disease can be communicated by the mother to the child *in utero*, but without obtaining any satisfactory information. Many old negro women have told me, that a child born under these circumstances never has the yaws, but no reliance can be placed on their testimony. Equally suspicious is any infor-

mation I have on the question, whether the air can propagate the disease. The subject is so open to fallacy, that I would place reliance on no conclusion. To determine at what time the eruption becomes infectious, and when it ceases to be so, would require more facts than what I am yet in possession of.

The practice now generally adopted in this island, is to leave the disease to the efforts of nature, to feed the subject with good diet, and do light work. In weakly children, towards the conclusion of the disease, the decoction of woods, flowers of sulphur, or antimonial preparations, are of considerable service.

Any proprietor of negroes is well aware of the loss he sustains from the yaws, not so much from the actual disease, as from the fatal symptoms that too often follow. The finest looking slave will, in consequence of such effects, in a few months become a burden to himself and his master. The yaws have their bitter dregs, as they are vulgarly called, as well as other diseases.

If the patient has been ill fed or caught cold, and the eruption repelled, he generally dies dropsical. When the disease has attacked the throat, the soft parts are always lost; repeated inflammations ensue; and, from being considered by the ignorant as venereal, the poor creature, to his present misfortunes, has that of a needless salivation added. Mercury is of trifling service. A regulated diet, and the use of the country remedies, is much more efficacious; but the most unpleasant consequence is, that, if there should be any tendency in the constitution to hereditary disease, it is universally excited into action. The patient, shortly after the eruption ceases, begins to complain of pain and stiffness of all the joints. He loses flesh, the bones swell, the voice becomes guttural, there are patches over all his skin, the feet swell, and repeated attacks cause them to remain so permanently; the bones are swelled and deformed, and, finally, some ulcer breaks out, and the patient is rendered an object for life. These, and many others, are called the effects of ill cured yaws, but I am convinced, with little foundation. Symptoms of a similar nature begin to show themselves in many negro children at or before the age of puberty; previously, we could not wish to see finer live-

lier creatures. They begin to pine away, worms are suspected, and remedies are administered without relief. A sore at last may break out, and the symptoms may here remain stationary, but more generally the disease goes on, and the patient becomes disgusting to every one. He lingers a few years in some lonely spot. By most people in this country these symptoms are called venereal, arising from *rotten* parents, as they are termed. My views of the venereal disease would never allow me to agree to such a doctrine; and, after giving the subject every attention, I am convinced we must look for their origin at a different source. They will be found to be leprosy's affections in varied forms, arising from the hereditary tendency that is known to exist in such diseases. The confusion that prevails in the history of leprosy, shows sufficiently the different forms that climate imposes upon it. The negroes were formerly very subject to it. From better living and other causes, the tendency has not been roused except in some particular instances; and that severe form of it mentioned by many authors is hardly to be met with.

* I am convinced that such cases are not venereal, because such severe cases are not known in this climate. The children at birth are healthy, and remain so many years; the parents are found often to have no disease, but a leprosy affection can be traced to some one more remote; the symptoms are not curable by mercury, and it only attacks particular children of a family. The leprosy children who derive the disposition from their parents, escape the disease till near puberty. Climate has varied the forms of the leprosy, witness the *coca bea*, or joint-evil. It has many of the characteristics of true leprosy; and has not climate so modified the virulence of the venereal disease, as to enable the constitution to overcome it without the aid of mercury?

I have often been astonished that mulatto children, generally born of healthy parents, are more subject to these symptoms than black children. I have often suspected, that, in the crossing of the different races of beings, such hereditary tendencies were more liable to be excited. I lately found this conjecture to be correct, from the information of an intelligent gentleman who resided many years in a Spanish colony

where leprous affections were very prevalent. He said that the natives have an old idea, that, if a man have connection with a black woman predisposed to leprosy from her family, the offspring will become diseased. This they prove by citing many cases of negro women whose children, by white men, were diseased, but whose future offspring, by husbands of their own colour, have remained perfectly healthy; and they attribute the exemption of the higher orders in society from these diseases merely to their avoiding the exciting causes.

Whether these remarks on leprosy are correct, others having more extensive opportunities must judge. My chief object in making them is to do away with the abominable practice of the indiscriminate use of mercury. It ultimately ruins the individual, while a more simple treatment would render his life comfortable.

The yaws has been compared with that disease mentioned by Moses in the 13th chapter of Leviticus. If medical men can reconcile them, it is much to their credit. In my humble opinion, that chapter requires a very ingenious commentator to explain it.

I have by me the notes of an old practitioner of this island, who went home to a part of Scotland many years ago, where the *sibbens* was prevalent. His remarks are made without any allusion to theory, and they tend to confirm the identity of yaws and sibbens in many particulars. Observations from a person who had seen both diseases are peculiarly valuable. It would be improper to lengthen this communication with entering on this point at present. I shall take another opportunity of offering you some farther researches connected with this subject.

Case of Survival of 'Twelve Days' Abstinence from Food, occurring in a Person in the state of Health. By THOMAS T. GRIFFITH, Member of the Royal College of Surgeons.

[From the London Medical and Physical Journal, for February, 1820.]

THE leading particulars of the following extraordinary case, having happened in part under my own immediate observation,

and in part under such circumstances as leave but little room to doubt their having occurred, I am induced to request a place for the insertion of them in your Journal.

On the morning of the 27th of last September, an immense body of water, which had accumulated in one coal-pit, suddenly burst into another, where nineteen men were at work; sixteen of them were enabled to extricate themselves from the perilous situation which they were thus unexpectedly placed in, at a distance from the surface of one hundred and twenty yards. But, such was the quantity of water, and the changes it rapidly produced in the pit, that the remaining three were cut off from all means of escape. In the hope of being able to rescue these unfortunate people, every measure that could possibly expedite the discovery of them was unceasingly pursued. On the ninth day the bodies of two, who had been drowned, were discovered, and on the twelfth day some vestiges of the third man were traced; in a short time his voice was heard; and soon afterwards he presented himself to the astonished workmen.

It appeared from his subsequent statement, that, when the water was rushing in an overwhelming stream towards the spot where he stood, he had only sufficient time to allow his gaining a situation above the highest level to which the water reached. In this place, which was a dark, low, close passage, communicating with two galleries, he remained for *twelve days*, without any other sustenance than a little water, which, trickling down a rock distant about twelve paces from the place where he lay, he collected in a hollow formed by his hand; to this he occasionally crept on his hands and knees, and at other times he relieved himself by merely changing his posture. He computed the period of his confinement to have been about *nine* days. For the first third part of this time he felt a considerable desire for food, which afterwards entirely ceased; and he only experienced an occasional painful sensation in the region of his stomach, which was always removed by drinking a little water. The greatest part of the time was spent in sleep, accompanied by dreams. Some of the functions of the body appear to have been nearly suspended, and others to have been very slowly performed. The bowels were reliev-

ed once only; there was no sensible perspiration; the breathing was slow; but the urine was voided in small quantities about twenty times. His mind suffered but little depression, except upon one point; in general, it was at ease, and he felt confident that at the end of a computed time the water would be removed, and a communication with himself be opened: but, when he reflected upon the destitute and unhappy state of his wife and children, the powers of reason yielding to a pleasing hallucination: he thought that celestial sounds were near him; every unpleasant feeling ceased, and happier and more animating views employed his thoughts.

At the moment he was found, and for some time afterwards, he declined taking any thing more than a little dry biscuit; he was therefore merely removed to a warmer part of the pit, where I first saw him. He was in full possession of his mental faculties, his spirits were buoyant, and his feelings alive to the hearty congratulations of his companions, and to the anticipated pleasure of returning to his family. His face and figure were much emaciated;* there was an expression of wildness and famine in his countenance; his cheeks were flushed; his tongue was slightly furred and white; his pulse was regular, about 112, but very weak and small; and his extremities were cold. He complained of some degree of thirst, but of no pain: he had no wish to take food.

Fearful of calling into sudden or violent action the accumulated irritability of the system, liquid food, of a mild nutritive kind, was administered to him in small quantities, and at stated intervals: although he took it with indifference, his stomach retained it. The circumstance of his deliverance, and his anxiety to see his family, excited his mind so powerfully, as to give an unexpected degree of tone and energy to his whole frame; of this I thought it right to avail ourselves, and to remove him from his present situation to some house, where we could better guard against the effects of that debility which might be expected speedily to result from his present state.

* On the fourth day from that on which he was found, he weighed twenty-eight pounds less than he had done some weeks previously. He is a muscular bony man, of the middle size.

He was brought up from the pit in a tub, so large as to admit of his lying down in it, lest a continuance in the erect posture might have induced syncope, under circumstances very unfavourable to his recovery. In this way he experienced very little inconvenience from his removal. During that evening, and the following night, he was ordered to take the yolk of egg, mixed with warm water and sugar, weak broths, gruel, milk, &c. in larger quantities and with longer intervals; and, once in three hours, part of a slightly-cordial mixture, containing a few drops of laudanum in each dose. The nurse was directed to wake him occasionally, if much disposed to sleep.

Sunday, (the following morning.)—He has passed a tolerably comfortable night, occasionally slumbering. He has taken some quantity of food without any sickness. He now feels much exhausted; his pulse is languid, and reduced from 112 to 60 strokes in a minute. He has no desire for food: faintness comes on in the erect posture. He has voided urine. A more nourishing and stimulating diet was ordered. In the evening he was better, but suffered some degree of uneasiness from flatulency and heat. As his bowels had not been relieved for some days, the following mixture was prescribed: *R Pulv. Rhei ʒij. Potassæ Tartr. ʒj. Tinct. Sennæ ʒij. Aquæ Puræ Infus. Rhei āā ʒiss. M. Capiat cochlearia ij. majora 3ra. quaque horâ, donec alvus respondeat.* The operation of the medicine produced four evacuations of hardened black fæces, and afforded the patient much relief.

Monday.—He is better; has passed an easy night with some sleep. His pulse is 84, and fuller. His appetite is returning. He complains of an unpleasant degree of heat in his feet. The same kind of diet is to be continued.

Tuesday.—This morning I was requested to visit him as soon as possible, as he was much worse. I was informed, that, finding himself better on the preceding evening, and his appetite greater, he had taken solid food, and ale in small quantities: in consequence of which he had had a very restless night; his skin had been hot and dry; his bowels distended and painful; his stomach oppressed; he had been thirsty, and at times delirious. Nor did these symptoms disappear, until the stomach had been relieved of its contents, and the bowels

had been acted upon by an aperient. Two or three days of restlessness, heat and languor, with diminished appetite, elapsed, before he recovered that point of convalescence which he had thus lost by his own temerity and imprudence. Such a degree of reaction took place in his feet and toes, as seemed for a short time to threaten the vitality of those parts.

During the ten or twelve ensuing days, a variety of symptoms, chiefly arising from the different states of the alimentary canal, appeared. For a day or two the pulse intermitted, but was restored to its regular action by emptying the bowels. Nothing material, however, prevented his ultimate recovery; and his general health is now, December 16th, perfectly re-established. His last complaint was of a severe rheumatic pain in the shoulder, on which he had lain during the greater part of the period of his confinement in the pit, and which he still continues to suffer at times.

Remarks.—It may be asked, “were there any peculiar or local circumstances which could have aided the powers of the system in supporting life, for so long a period, under a deprivation of food, so entire?” I believe that, providentially, there were. The man’s age 27; his bodily strength, his habits, and that self-possession and strength of mind which led him to an almost correct calculation of the time required for the removal of the water and other obstacles, and enabled him to wait patiently the expiration of that time, in a confident hope of being ultimately rescued, were advantages greatly in his favour. But there existed also a local cause,—the peculiar kind of air which he breathed; and which, I am disposed to think, contributed materially to the preservation of life. From repeated examinations of the air in the pit, from my own feelings and observations whilst below, and from the man’s description, it was ascertained that, with the water, a quantity of carbonic acid gas had diffused itself through the lower parts of the pit. The situation which the man occupied was an elevated one, and would therefore raise him above the more concentrated stratum of carbonic acid gas; but he was still aware that the surrounding air was heavy and oppressive: he said, “that for the greater part of the time he breathed the *damp* air, but that it became better as the water lowered.” To enter again upon

the particulars of the case, or to reason upon the known properties of carbonic acid gas, and its effects under different modifications, would extend these observations beyond their proper limits. From the positive evidence of the man having breathed an atmosphere containing a greater or less proportion of carbonic acid gas, for twelve days, and from his having survived, may we not infer, that this gas, by producing a sedative effect on the system, by moderating irritability, and restraining action, prevented the active exercise, and consequent exhaustion of the vital powers, and thus proved conducive to the preservation of life?

Experiments on a few controverted points respecting the Physiology of Generation. By JAMES BLUNDELL, M. D. Lecturer, in conjunction with Dr. Haighton, on Physiology and Midwifery, at Guy's Hospital.

[From the Medico-Chirurgical Transactions, Volume X.]

AMONG the various questions which have been raised respecting the generation of animals, there is one, as yet undecided, which has not perhaps been hitherto investigated with all the care it deserves. It may be demonstrated by experiment, that, in this curious process, the male furnishes the semen, and the female the rudiments; but whether these two substances must have access to each other, in order that the young animal may be formed, is a question which still admits of dispute. It is true, indeed, that many naturalists have asserted, that contact is necessary; and Spallanzani has even gone so far as to demonstrate that it certainly takes place in the generation of the frog and toad. Still, however, notwithstanding the labours of physiologists hitherto, we are not, I believe, as yet in possession of any regular system of experiments, which proves that the semen must have access to the rudiments, in those forms of brute generation which most nearly resemble our own. In the present state of our knowledge, the reverse of this position seems, at least, not improbable, as the experiments of Dr. Haighton, a valued relative

of mine, have shown, that evidences of generation may be produced in the ovaries, although the semen has been excluded previously to sexual intercourse by the closure of the fallopian tube.

The principal object of the memoir, which I have now the honour of presenting to the Medico-Chirurgical Society, is to contribute some little towards the supply of this defect. In it I have endeavoured to show, that the semen must have access to the rudiments, in order that the young animal may be produced; and yet, that generation, although these approaches are necessary for its completion, may, to a certain extent, be accomplished without them.

As the rabbit was the animal, on account of its natural aptitudes, selected for my experiments, it may be proper, perhaps, before I enter on the recital of them, to premise a few remarks on its genital system.* In the fallopian tubes, and ovaries, and, I may add, the *external* genitals of the doe, there is little, when we view the organs as they are suspended in the glass, to attract the attention of the observer. It is different, however, with the vagina and the wombs; these are so strongly contrasted with the corresponding parts of the human organs, the wombs, by their tubular form, and the vagina by its length, its laxity, and large diameter, that they cannot be overlooked.

The vagina, when full grown, is about four inches long, and so capacious that, without much stretching, it will readily admit the extremity of the fore-finger. Its size, indeed, is so considerable, that it makes an approach to that of the human vagina, and greatly exceeds the dimensions of the same canal in a moderate sized monkey, preserved in the obstetric Museum, at Guy's Hospital.

The wombs, the structure of which is scarcely less remarkable than that of the vagina, are two tubular organs, when unimpregnated, about three inches and a half long and about two lines and a half in their diameter; they are therefore, it is obvious, very unlike the human uterus, and rather resemble that

* It is scarcely necessary to remark that this description is not addressed to those who have made a study of comparative anatomy.

of several of our domesticated animals, as the cat, for instance, the bitch, and the females of the rat and mouse tribe. These two wombs, it should be further remarked, communicate with the vagina by two distinct orifices; and they are so completely independent of each other, that the one may be removed, without injury to the other, excepting a slight and superficial wound of that part where their necks lie in contact, and cohere.

Both the wombs and the vagina are, in these animals, furnished with longitudinal and annular fibres of a muscular structure, similar in kind to those of the intestines, but grosser and more distinct. In addition to these, along the inner margin of the wombs, from one extremity to the other, there runs a broad strip of fleshy fibres, which may, perhaps, not improperly be denominated the mesometric. I give the muscle this name, because it covers no inconsiderable portion of what may be called the *mesometry*;* a delicate double membrane, the production of the peritoneum, which performing, for the tubular wombs, the office of a mesentery, unites them, like the intestines, to the chine. It is allied to the broad ligaments of the human womb.

All these fleshy fibres are animated with a very lively irritability. The mesometric muscle† changes the situation of the wombs. The wombs themselves perform a peristaltic action. The vagina not only performs this action, but an additional movement, which I shall hereafter have occasion to describe.

Such are the most striking characteristics of the genital system in the rabbit, those, at least, which the following experiments require me to notice. I may now proceed to the experiments themselves.

The first set of experiments was instituted with a view of ascertaining whether the semen and rudiments must have access to each other, in order that the young animal may be formed. For this purpose, an incision was made into the cavity of the belly, immediately above the wombs; and these,

* I venture on the name with diffidence, but no preferable term occurs to me; its etymology is obvious, and I believe, legitimate and analogical.

† Is this muscle allied in function to the round ligaments of the human womb?

together with the upper part of the vagina, were pushed through the opening. One of the wombs was then divided near its mouth, in a transverse direction, just as a piece of intestine might be, so as to separate it into two portions, the superior and inferior; or, as they may be designated from the annexed parts, the vaginal and fallopian. After this division the organs were immediately replaced, and the wound was sewed up.

Notwithstanding this violence, in the course of a few days, or a few weeks at farthest, most of the rabbits recovered their health, and at different intervals became fit for the approaches of the male. But though the general health was restored, the recovery was not complete. The operation, as subsequent dissection proved, had the effect of interrupting the canal of the womb, its tubular cavity growing up at the line of division, so that the communication between the vaginal and fallopian pieces became intercepted, and the semen and the rudiments could have no access to each other.

In this condition of the genitals, as soon as the sexual ardour was rekindled, the animals were submitted to the male; and, excepting in one or two anomalous instances, out of ten or twelve experiments, they all became pregnant from the *first* admissions. At different periods from impregnation the sexual organs were examined after death with great care and deliberation, when young animals were invariably found in the sound womb, but none in the interrupted. This, it is true, like the human uterus in extra-uterine pregnancy, was in many instances enlarged and developed and plentifully supplied with blood: indeed it often appeared as well adapted as its fellow for receiving and cherishing the rudiments; but with all its aptitudes for generation, it lay under one capital defect, its canal was interrupted; it intercepted the access of the semen to the rudiments, and without this access, generation could not be accomplished.

To confirm this conclusion, the accuracy of which I doubted at the time, it was determined to submit it to the test of another train of experiments. In these it was my object, to preserve the principle of the preceding operation, the exclusion of the semen from the rudiments; and yet at the same

time, to vary its circumstances as much as possible, in order to ascertain how far they had affected the result; for I need not observe, that circumstances often exert a silent and most fallacious influence over our experiments, our negative experiments especially, to be deprecated the more, because, from its insidious nature, it is so frequently overlooked.

In this second series of experiments, therefore, instead of operating upon rabbits that were full grown, I made use of those only that were under their puberty; and instead of interrupting, as before, the canal of the *uterus*, I interrupted that of the *vagina*.

The vagina of the doe, it has been already observed, is at least three inches in length; so that although it is interrupted at the uterine extremity, there still remains sufficient room for the male organ. Of this peculiarity I availed myself, in conducting these experiments; and instead of cutting the uterus, I cut the vagina asunder, near to the mouth of the womb, so as completely to interrupt its canal. In other respects the experiment was conducted as before.

This operation proved dangerous, much more so than the former; a number of the rabbits however recovered, and admitted, without repugnance, the approaches of the male. The result was decisive. Although the external genitals of these animals were turgid with blood, and the sexual excitement of some was remarkably lively; although too, in some of them, intercourse was renewed at intervals of a week or a fortnight, on the whole, as many as twenty or thirty times, not one became pregnant. Desire itself, in one or two instances, seemed almost insatiable; and in the rest, though suspended by coition for a time, in the course of a few hours, or a few days at farthest, it invariably recurred.

The same general appearances were observed on dissection, in them all. The vagina, if the operation had been properly performed, was completely interrupted. In both the ovaries there were *corpora lutea*. In some cases, the wombs appeared to have undergone little change; in others, they were enlarged, and evolved as completely as in actual pregnancy; but in no one instance was there the appearance of a single *ovum*, extra-uterine, or in the womb. In these, as in the preceding ex-

periments, though in a different manner, the access of the semen to the rudiments had been intercepted, and under these circumstances, notwithstanding repeated commerce with the male, the formation of the young animal could not be accomplished.

In performing the experiments recorded in the preceding paragraphs, there are various little niceties in the mode of operating, the observance of which is necessary to ensure success. The incision which is carried through the abdominal coverings, may be made in the *linea alba*, and should be eight or ten lines, at least, in length, in order that the parts may be replaced with facility. It should, too, lie as close to the *symphysis pubis* as possible, that the intestines, which in this herbivorous animal are numerous and cumbersome, may not, as they are apt to do when the incision is higher, protrude at the opening. It is true, indeed, that if the incision is placed in the vicinity of the pubes, the bladder, when it is distended, will fall in the way; but if the operator possess the requisite dexterity, there is no danger of wounding it; and a gentle pressure, persevered in for a time, will occasion it to withdraw into the pelvis. It deserves remark, however, that to produce this contraction, a little perseverance is necessary; for the bladder is not, in this manner, so readily excited to contract, as from previous reasonings on its irritability, we might have been led to expect.

To close the abdominal opening, the glover's suture will serve as well as any other; nor does the including the peritoneum in the stiches, so far as I have been able to observe, materially increase the risk of a general inflammation. Exemption from this, depends much more upon the habit of the animal, than the niceties of the wound.

And here I may be permitted to remark, in the way of digression, that from various observations* upon brutes, as well as upon my fellow-creatures, I cannot forbear imagining that the

* Operations for hernia and on the abdominal viscera of rabbits and dogs. The rabbit I suspect is very liable to spontaneous inflammation of the bowels. I have known in women the malignant ulcer of the womb penetrate into the peritoneal cavity, between the *rectum* and the *uterus*, without exciting a general inflammation of the belly.

risk of extensive inflammation, from local injury of the *peritoneum*, has been exaggerated, perhaps greatly. The high importance of this principle in surgery, is too obvious to require a comment; already a sufficient number of observations has been accumulated, to induce us to examine it with attention; and I may add, that it is one of those grand practical points, which ought not to be decided by a few casual facts, much less by authorities, however venerable; but, like every other principle of a solid philosophy, by various, deliberate, and unbiassed experiment and observation.

If, in performing this operation, as in the first set of experiments, the womb is divided, the incision should be made transversely near its mouth, in order that we may leave the fallopian piece as large as possible, for the reception of the *ova*, in case the genitals should have power to form them. It ought, too, to be carried from four to six lines into the mesometry, in order that the pieces thus liberated, and moving out of apposition with each other, may not reunite so as to form anew a continuous canal. If, on the contrary, as in the second scheme of experiments, the vagina is divided, a ligature should be applied to the orifice of that piece of it which remains annexed to the womb, and fastened to the margin of the external wound. This precaution ensures the escape of the thread,* and at the same time prevents the pieces of the vagina from falling into apposition, and renewing the continuity of the canal.

When the genitals are mature, the rabbit very frequently dies from this operation; which, in consequence of the large size of the vagina, is more violent than the former. It is better, therefore, on this account, as well as for reasons already assigned, to operate before puberty. Previously to this change

* In operating upon the viscera of small animals, I have occasionally used a very slender ligature, have cut it short, and left it. In two rabbits, which had apparently recovered after the vagina had been tied in this manner, a general inflammation of the belly came on about six months afterwards, in the winter, when the health of the animals was impaired by the severity of the season. On inspection after death, it was found, that the ligature still adhered to the vagina, and it seemed to form the centre from which the inflammation had spread.

the parts are comparatively small, and the interruption of the vagina does not, as we might have been led from previous reasonings to expect, prevent the subsequent developement of the sexual organs. But to return from these details.

Although it appears probable, from the preceding experiments, that the complete process of generation requires the access of the semen to the rudiments, it seems equally certain, from a variety of appearances which I noticed in the course of my experiments, that to a certain extent, though imperfectly, it may be accomplished without it. These appearances I shall now proceed to state.

In both the uterine and vaginal experiment, the womb, though it contained no fetuses, in many cases enlarged, as in extra-uterine pregnancy. Its structure too became developed; it received more copious supplies of blood; in short, it frequently seemed as well prepared as its fellow, for receiving and cherishing the rudiments.*

The ovaries, too, I may further add, although there was no genuine impregnation of them, were very obviously excited. The vesicle in different parts of them germinated; its fluids increased; the delicate covering opened; the little cavity discharged its contents, and corpora lutea formed in all their perfection. As this appearance of the *corpus luteum*, notwithstanding the interception of the semen, is of considerable importance, and may help to clear away an objection to which the experiments lie open, it becomes necessary to examine it with attention.

The corpus luteum in the rabbit, as long as it remains, is, I think, *always* marked by pretty strong characteristics, though its appearance differs considerably with its age. A mammillary projection of the ovary, an augmented vascularity, a minute cavity, which, when the luteum is cut through, recalls to mind the appearance of a printed asterisk (*), constitute the leading characteristics; and by these, I may add, it is so decisively marked, that, although the parts are on a small

* It deserves notice, that, in the uterine experiments, it was generally the fallopian portion of the womb to which the semen was not applied, and not the vaginal, to which it was applied, which appeared to undergo these changes in the highest degree.

scale, an experienced eye may detect it at a glance. Colour is of little use in distinguishing these bodies in the rabbit. The younger the *luteum* is, the more prominently the characteristics appear.*

Now, these lutea, thus characterized, were distinctly produced both in the uterine and vaginal experiments. In the uterine experiments I had an opportunity of contrasting those of the fruitful and sterile ovary with each other, and yet, after the most deliberate examination, I could not discriminate the slightest difference between them. It deserves notice, also, that in some instances they were more numerous upon the prolific, and in others upon the barren side of the genitals.

In these experiments, it may be further remarked, the fallopian tubes, as well as the ovaries and wombs, seemed to be excited by coition. I observed repeatedly, in those experiments in which the vagina was interrupted, that the abdomen of the doe enlarged in a few days after the sexual commerce; and that enlargement, never noticed before, and gradually decreasing† in a few weeks afterwards, if the male was excluded,

* In giving the name of *corpus luteum* to the appearance here described, I merely adopt the nomenclature of preceding physiologists; and in stating my belief that this appearance is the result of impregnation, or, at most, of the sexual excitement when exalted to its highest pitch, I am only advancing an opinion, which is, I conceive, *as far as respects the rabbit*, confirmed by observation. I have frequently examined the ovaries of the doe, in the virgin condition, and during heat; and in one or two cases, after the animal had been under the influence of long continued and lively desire. In the two last instances I have never found the appearances described, though I dare not, from a negative observation of this kind, deny, that, under these circumstances, their formation is possible. In the first case, on the contrary, I have invariably discovered them, and older or younger in their appearance, according as they were examined sooner or later after impregnation. There can, therefore, I apprehend, be little doubt, that these appearances occurring *in the rabbit*, are the result of conception. This fact is sufficient for my reasoning. It may, indeed, seem irreconcilable with the opinion which a veteran physiologist has formed, respecting the nature of the human *corpus luteum*; see Philosophical Transactions; but so long as it appears to be confirmed by observations, conformably to sound philosophy, it cannot be denied. I am far, however, from wishing rashly to impugn the opinion of Sir Everard Home. Truths once proved must be admitted, and their apparent inconsistency demonstrates our ignorance, not their incompatibility.

† It did not however subside completely.

might by repeated coitions be carried to a very great degree. There is now in my possession, a doe with an interrupted vagina, which has admitted the male from twenty to thirty times. In this animal, in consequence of these repeated connections, the abdomen has gradually acquired so large a size, that it considerably exceeds the bulk of mature gestation, and reminds one of the tumour of an ascitic which requires the trocar. These enlargements, I have ascertained from repeated dissections, result from the accumulation of a humour in the wombs. This humour, various in its consistency and colour, is, however, generally fluid and pale, and turbid; and always, so far as my experiments have extended, forms albuminous concretions at a temperature below boiling heat. Even in the uterine experiments, for the preceding remarks refer to the vaginal only, the same essential appearances were observed; the wombs, in consequence of impregnation, became filled, on the sound side, with fetuses, and on the barren with the humour described.

These facts are very significant. The formation of the lutea, the developement of the wombs, and above all, the repeated accumulations of fluid there, in consequence of coition, all seem to indicate the descent of the rudimental material; and reflecting upon them, I cannot forbear imagining that the tubes were excited, that they really transferred the rudiments to the womb, and that these rudiments engendered the watery accumulations there, in the abortive attempts of generation. This notion receives some little countenance from the generation of oviparous animals; for in many of the different species referred by naturalists to this class, the rudiments may be discharged independently of preceding impregnation. The common fowl is an example of this; the frog, the toad, and a numerous tribe of fishes. This opinion, however, is merely conjectural, and I must acknowledge candidly that it is the less entitled to confidence, as it rests on a sort of accidental observation, made subordinately, perhaps with some degree of remissness, at a time when others of greater importance in the inquiry occupied a principal share of my attention. This remark I take the liberty of introducing here, as I conceive it to be the duty of every experimental inquirer *himself* to dis-

tinguish between his conjectures and demonstrations; and thus, by the exercise of a philosophical frankness, to prevent error from insinuating itself from its association with truth.

On the whole, then, it seems probable, judging from the appearances related, that generation may be carried forward to a certain extent, although the access of the semen to the rudiments is intercepted. Under these circumstances, the young animal cannot be formed, it is true; but corpora lutea may be generated; the wombs may be developed; and the rudiments, if we may judge from the facts already stated, may even be transferred to the uterine cavity by the play of the fallopian tubes.

It should be remarked, however, in dismissing this part of our subject, that these imperfect attempts at generation do not always equally occur. Corpora lutea, I believe, will be found to form invariably after sexual intercourse, if the genitals are excited at all; but in some anomalous instances, there is no consequent developement of the wombs, and in others, no accumulation of the uterine fluid. The first of these failures has occurred to me once in twelve experiments, and the last of them five times.* But these *negative* irregularities merely prove, that, under circumstances, the genitals may be more extensively excited at one time than another. They by no means invalidate the principle which it has been my endeavour to establish on positive facts, that the ovaries, tubes, and uterus, are capable of an imperfect excitement, even when the semen and the rudiments are kept apart from each other.

Against the experiments and reasonings advanced in the preceding pages, various objections may be urged, to which it may now be proper to advert.

And first, it may be objected that sterility is sometimes an accidental occurrence. We frequently observe it in human generation. In the experiments under consideration it would perhaps have occurred, although the interception of the semen, to which it is ascribed, had not taken place. To these objections, however, I would reply; that in the rabbit the acciden-

* In one or two instances the orifice formed by dividing the uterus remained open in the fallopian piece. This accounts for some of the failures of uterine accumulation.

tal failure of impregnation is rare, and does not occur in one doe out of twenty, if the animal is in health; that the appearance of the genitals, and the behaviour of the female when the male was admitted, both of them indicated inclination and aptitude for generation; that these experiments were not solitary, but frequently repeated; and that sterility was not an accidental occurrence, in a single instance only, but an invariable result of them all. Nor must it be forgotten that the formation of the *lutea*, and the evolution of the *uterus*, are themselves sufficient proofs that the genitals were not accidentally inactive; nor that in the uterine experiment, in which the semen was intercepted on one side only, there were undeniable proofs of the generative excitement in the formation of the young animals on the other.

But there is another objection to which the experiments lie open, which, on a cursory consideration, at least, may appear to bear with considerable weight. In these operations either the wombs or the vagina were cut asunder. It may be asserted therefore, that sterility ensued, not so much in consequence of the interception of the semen, as from the debility induced in the genitals by operative violence; the germs afterwards perishing because the soil was become unfriendly.

To this plausible objection, however, it might be sufficient to reply, that from the form of the parts the injury of the operation is merely local; that when the vagina is cut through, before puberty, the genitals suffer so little from it that they are afterwards brought to maturity in the same manner as if no operation had been performed; and that in both sets of experiments, whether uterine or vaginal, the wombs frequently become enlarged and developed, and like a fruitful and well dressed soil, to resume the figure already adopted, are brought into high condition for raising the rudiments to perfection. To obviate this objection, however, in a still more satisfactory manner, the following experiments were instituted.

I divided the vagina of two young does, just before their puberty; but instead of securing the uterine piece to the verge of the abdominal wound, I allowed it to remain in apposition with the other. In consequence of this method of operating

the parts reunited; the canal of the vagina was renewed; and the sexual desires appearing a few weeks after recovery, both the rabbits became impregnated. The inference is obvious.

The second set of experiments, turning on the same principle, was executed on the wombs themselves. In these, both the wombs were divided, the one in two, and the other in three places; in such a manner, however, that the incision was not carried completely across into the mesometry; so that the pieces were retained in mutual apposition, and reunited without interruption to the uterine canal.

The result of these experiments was decisive. From the very method of operating it is obvious the wombs were more roughly handled in this than in any of the preceding experiments; accordingly a larger number of the rabbits died; and yet, notwithstanding this violence, the very first doe which recovered produced no less than nine fetuses from her *first* intercourse with the male. Indeed so complete was the action of the *uterus*, that there was not one of the little masses of rudimental matter which it failed to mature; and it was found, on a careful comparison of the wombs with the ovaries, that the number of fetuses and corpora lutea was the same. To these remarks I may add, that the human womb, although it has been cut or torn, or partially destroyed by ulceration, still retains the power of maturing the rudiments. Healthy children have been born, not only after recoveries from uterine rupture and the Cæsarian operation, but even at the time when the neck of the womb had been ulcerated. A case of this kind has lately fallen under my own knowledge; and others are recorded by obstetric writers.

There yet remains a third objection, which, it is conceived, may be completely obviated, though at first view it wears a very formidable aspect. The vagina of the rabbit is very long and very large; its course is not direct; the organ of the male can neither fill it nor penetrate to the orifices of the wombs; how then can the semen be injected into the uterine cavity, even granting that it might meet the rudiments there?

This objection, felt in all its force by those who have examined the genitals merely in the preparation glass, falls at once when they are viewed in the rabbit while it is living; or,

to avoid unnecessary severity, immediately after the dealer has killed it.

Both the vagina and the wombs perform a peristaltic action, the wombs somewhat obscurely, the vagina in a more lively manner, even than the intestines of the animals themselves. This canal indeed, during the heat, is never at rest; it shortens, it lengthens, it changes continually in its circular dimensions; and, when irritated especially, will sometimes contract to one third of its quiescent diameter. Now this peristaltic action, resembling the intestinal, is itself sufficient to explain the transmission of the semen.* In addition to this action, however, the vagina performs another, easily comprehended on inspection; although, as frequently happens, the verbal description of it may perhaps appear a little obscure. The action to which I here allude, consists in the falling down, as it were, of that part of the vagina which lies in the vicinity of the wombs; so that it every now and then lays itself as flatly over their orifices as we should apply the hand over the mouth, in our endeavours to stop it. So close is this application, that I have sometimes fancied I could perceive externally something resembling a little dimple, occasioned by the sinking of the surface of the vagina into the orifice of the womb. How well adapted the whole of this curious movement is for the introduction of the semen at the opening, it is needless to explain. The mere performance of it furnishes no contemptible argument in proof of that approach of the semen to the rudiments for which I have been contending.

Before I close these observations, already perhaps too diffuse, I cannot forbear adverting to some other points of the genital physiology, which they may contribute to illustrate.

It has been asserted by some naturalists, that the corpus luteum is an evidence of genuine impregnation. It seems *certain*, however, from the facts related, that this evidence cannot be relied on; for the luteum, in these experiments, was

* There is some little reason for surmising that even the human vagina can perform a sort of peristaltic movement. Two facts have been related to me which lead to this opinion, but they are of a character too delicate for public exposure.

generated under circumstances in which, as the event proved, impregnation was impossible. Indeed there seems to be little reason for doubting, that the corpus luteum may be produced, even independently of the sexual intercourse, by the mere excitement of desire in a very high degree. Mr. Saumarez has recounted experiments, in his "New System of Physiology," in which the *luteum* appears to have been generated in this very manner. I have now in my possession a preparation, for which I stand indebted to Dr. Cholmeley and Mr. Callaway, consisting of the ovaries of a young girl, that died of chorea, under seventeen years of age, with the hymen, which nearly closed the entrance of the vagina, unbroken. In these ovaries, the corpora lutea are no fewer than four. Two of them, it must be acknowledged, are a little obscure; though an experienced eye, I conceive, would readily detect them. The remaining two are very distinct, and differ from the corpus luteum of genuine impregnation, merely from their more diminutive size, and the less extensive vascularity of the contiguous parts of the ovary. In every other respect, in colour and form, and the cavity which they contain, their appearance is perfectly natural; indeed so much so, that I occasionally circulate them in the class-room, as accurate specimens of the luteum upon the small scale.

On this point I have been the more explicit, both as the principle is of some importance in forensic medicine, and as it removes at once an objection to which these experiments lie exposed, and which is taken from those of Dr. Haighton. In these experiments, very ingenious, and extremely beautiful, my valued relative has shown, with his usual accuracy, that the corpus luteum may form though the fallopian tube has been obliterated in some part of its course, and the access of the semen to the rudiments therefore has been intercepted. When, however, he infers from this, in opposition to the principle asserted in this memoir, that the ovary has been impregnated, notwithstanding the interception of the semen, he certainly falls into one of those errors, from which the most wary physiologist is never absolutely exempt, for the corpus luteum is not a certain evidence of impregnation.

The appearances related, I may further remark, afford,

when combined with others, a plausible proof that the semen sometimes penetrates as far as the ovaries; a point which has been much controverted.

In the varieties of human generation, we sometimes meet with extra-uterine pregnancies, in which the ovum not only lodges in the tubes, or the peritoneal cavity, but in the ovary itself. Indeed, this form of the disease seems on the whole the most common. Now, if it be true, as I have endeavoured to prove, that the young animal cannot be formed unless the semen have access to the rudiments, it is evident, that in these pregnancies, in which the fetus is generated among the graafian vesicles, the semen must have made its way up to the ovaries themselves. It must not, however, be too hastily inferred from this, that the semen always penetrates into these remote recesses of the genitals. Facts have been related, which give a shade of probability to the conjecture, that without the contact of the semen the rudiments may sometimes descend into the uterus; and certainly, although the opinion is not without its difficulties, it is not impossible that they may meet each other there.*

There is yet a third point in the physiology of generation, which the preceding experiments may contribute to elucidate. It has been contended by some naturalists, and not without show of reason, that the semen in generation is transferred to the blood-vessels; and as the purgative or emetic, when injected into the veins, exert their peculiar influences on the stomach or the bowels, so also, in their opinion, this active fluid, transmitted by the absorbents, makes its first impression on the vascular surface, and its second, by a similar sympathy, on the genitals themselves. What effects might be produced by injecting the semen directly into the veins when the genitals are in a state of excitement, I shall not venture to determine; as yet I am in possession of no decisive experiments upon the point, and it would be a mere waste of mind to speculate without them. It seems evident, however, from the facts related, that after transmission through the absorbents and their glands,

* Is the transfer of the semen beyond the womb the cause of extra-uterine pregnancy?

the semen retains no such generative influence. It will be readily conceded, that when a rabbit admits a large male, in vigorous health, and in the flower of its age, as many as twenty or thirty times, a large quantity of the genital fluid must be imbibed by the absorbents of the vagina, yet neither in the uterine nor the vaginal experiment, in which these repeated coitions sometimes took place, was impregnation by absorption accomplished. The simple exclusion of the semen from the rudiments always prevented the formation of the young animal; in the vaginal experiments it was not produced at all; in the uterine it was formed on that side only where the womb remained pervious.

On a review of the whole inquiry, it will, I conceive, appear not improbable that, for the completion of generation, the semen must have access to the rudiments; and yet that notwithstanding the necessity of these approaches, for its completion, the process to a certain extent may be accomplished without them. These are the two leading propositions which it has been my endeavour to establish; at the same time I have subordinately attempted further to show, that the corpus luteum is not a proof of genuine impregnation; that the semen, at least occasionally, penetrates as far as the ovaries; and that however copiously this fluid may be absorbed into the vessels, it is incapable of giving rise, by any impression there, to the complete circle of the generative actions.

Whether these principles of brute generation may be transferred to our own, I will not venture to determine. Analogical arguments, generally the best that physiology furnishes, are, it must be admitted, never absolutely demonstrative; but as the generation of the rabbit, in its other principles, resembles that of the human female, there seems to me but little reason for supposing that there is an essential difference here.

On the Doses of the Ancient Physicians. By E. MILLIGAN,
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[From the Edinburgh Medical and Surgical Journal, for April, 1820.]

HAPPENING, during last summer, to look into some ancient writers on medicine, my attention was particularly attracted by a feature of their practice of which I can no where meet with a satisfactory explanation. As the subject seems curious, it may, perhaps, be worth while to notice briefly in this Journal the results of a hasty inquiry into its origin and reality. To those possessed of more ample means of information they may appear jejune and puerile: and if any such condescend to correct their errors, by the same channel in which they are now humbly offered to their notice, I shall candidly acquiesce in this imputation.

The difficulty here alluded to occurs in the *doses* of medicines exhibited by the ancient physicians. These have long been observed to be very large;* and it was on remarking the impossibility, according to our ideas, of exhibiting them with safety to human life, or even of escaping destruction under their influence, that my observation was first drawn to the subject of ancient posology.

Dioscorides, who lived under Nero, and who, as he informs us, had attended to the subject of the *materia medica* from his earliest years, may be taken as indisputable authority on this point; especially, as we are informed by Suidas, that he practised as well as studied medicine;† and his work stood in such high estimation among the ancients as to be copied literally in many parts by Pliny, though almost a contemporary. Indeed, whoever peruses his own excellent work on the *materia medica* with attention, will seek little farther for proof of his accuracy and diligence; and he and Pliny must always be cited as examples of the triumph of genius and industry over the harassing toils and interruptions of a military life.‡ It was not the intention of Dioscorides to deliver formally the dose of each medicine along with its natural history and

* Arbuthnot on Ancient Weights and Measures, p. 286.

† By himself, also indirectly, De ver. albo. lib. iv.

‡ Dios. Praef.

medical application, and, therefore, it is only occasionally that we find him particularly describe the quantities and forms in which they must be exhibited. He, indeed, explains in one place why the doses were not comprehended in his work; and as the passage leads to a curious fact in the history of physic, we shall deliver it in his own words. Whilst treating of the white hellebore, a most important article of ancient materia medica, he adds, "Mensura dispensatioque ejus ab HIS tradita est, QUIBUS præcipuum fuit studium de eo (περὶ τῆς δόσεως αὐτῆς,) docere. Nosque Siculo ab Ethna Philonidæ in ea re subscribimus, longum enim fuerit materiam medicam ex proposito tractantes, faciendae medicinae rationem docere."* Hence it appears that the ancients carefully separated the posological part of medicine from the materia medica; and with good reason; as, besides being too bulky an addition to the multifarious information of that science, it would have put the more ostensible part of the art into the hands of the vulgar, as the uninitiated of those times dabbled quite as much in physic as they do at present. We see likewise that they were in possession of distinct treatises on the subject of posology, and that the person here mentioned, Philonides the Sicilian, was an eminent author in that department. This passage likewise explains to us why Hippocrates, Celsus, Dioscorides, Pliny, and all the earlier writers on medicine, so rarely mention the dose even of the most powerful medicines; and, without some such key to their motives, Dr. Arbuthnot† might well wonder at Hippocrates so carefully dosing his slops and asses' milk, whilst the most acrid remedies are apparently left to the discretion of the practitioner or his patient. I shall, however, in what follows, endeavour to throw some additional light on this peculiarity of the ancient physicians, though, from the above, it is evident that a reference to books of posology was always implied in the passages where the quantity and circumstances in which a medicine should be given are not specified by these authors.

Wishing to discover, if possible, whether some general

* Dios. Lib. IV. de veratro albo.

† Ancient Weights and Measures, 282.

multiple of our present doses might not be detected in the doses of the ancient physicians, I selected from the work of Dioscorides, all the medicines of whose composition and identity no doubt can be entertained, noting particularly such as must, when given in excessive quantities, produce the most violent effects. The history of each of the *latter*, as given by Dioscorides, was then carefully inspected, and the doses, with their proposed object written down. A medium of them all, if not already given by the author himself, was afterwards computed; and a corresponding medium dose, at the same time, extracted from the posological table given by Dr. Duncan, junior, in his excellent Dispensatory. The results were set down in the following table, wherein the drachm is taken at sixty-two grains, as ascertained by the famous antiquarian Greaves,* and admitted by Dr. Arbuthnot† as nearly correct. To enable the general reader to compare these measures of the text with the modern troy weight, another table is subjoined, containing most of the medical weights and measures anciently in use.

Comparative table of Ancient and Modern Doses.

ALOES. In haemoptoe, cochl. ij. In icterus, ob. iiij. ad ℥i.; for free purging ℥iij.

In grains 310 31 62 186 medium 62, mod. med. 15, but $\frac{62}{15} = 4.133$

SULPHATE OF COPPER, for worms ℥i.

In grains 62 0 0 0 ——— 20 ——— 5 — $\frac{20}{5} = 4.000$

ELATERIUM, common dose, ob. j. smallest dose, ob. ss.
to infants, chalc. ij.

In grains $10\frac{1}{3}$ $5\frac{1}{6}$ 2 0 ——— $10\frac{1}{3}$ ——— 2 — $\frac{10\frac{1}{3}}{2} = 5.166$

OXIDE OF COPPER, as an expectorant, ob. iiij.

In grains 31 0 0 0 ——— 20 ——— say 5 — $\frac{20}{5} = 4.000$

SCAMMONY, for a purge, ℥i. aut ob. iiij. for a laxative ob. ij.

In grains 62 $41\frac{1}{3}$ $20\frac{2}{3}$ 0 ——— 30 ——— 8 — $\frac{30}{8} = 3.750$

* Dissert. on Roman Denarius.

† Ancient Weights and Measures. He makes it $62\frac{2}{3}$

SQUILL, to purge off bile, ob. iij.

In grains 31 0 0 0 medium 10, mod. med. 2 but $\frac{10}{2} = 5.000$

BLACK HELLEBORE, to purge off bile or pituita, ʒi, or
ob. iij.

In grains 62 31 0 0 ——— 42 ——— 15 — $\frac{42}{15} = 2.800$

7)28.849

Mean quotient, 4.121

*Table of Ancient Medical Weights and Measures, reduced to
their equivalents in Modern Troy Weight.*

1 sextons = 1 obolus = 10 chalci* = $10\frac{1}{3}$ grains.

1 drachm = 1 denarius = 6 sextantes = 6 oboli = 60 chalci =
62 grains.

1 uncia = 3 cochlearia = 7 drachms† = 7 denarii = 42 sextan-
tes = 42 oboli = 420 chalci = 434 grains.

1 cyathus = 4 cochlearia = 10 drachms = 10 denarii = 60 sex-
tantes = 60 oboli = 600 chalci = 620 grains.

1 cochleare = $2\frac{1}{2}$ drachms = $2\frac{1}{2}$ denarii = 15 sextantes = 15
oboli = 150 chalci = 155 grains.

1 acetabulum = 15 drachms = 15 denarii = 90 sextantes = 90
oboli = 900 chalci = 930 grains.

1 hemina = 60 drachms = 60 denarii = 360 sextantes = 360
oboli = 3600 chalci = 3720 grains.

1 sextarius = 2 heminae = 120 drachms = 120 denarii = 720
sextantes = 720 oboli = 7200 chalci = 7440 grains.

1 congius = 6 sextarii = 12 heminae = 720 drachms = 720 de-
narii = 4320 sextantes = 43200 oboli = 43200 chalci = 44640
grains.

The candid reader will easily perceive, that the numbers
given in the former table as modern medium doses are not
meant for *moderate* doses of the medicines there mentioned;
they are merely the arithmetical mean of the extremes given

* My reasons for dividing the obolus into ten chalci, and not into six, as
Arbuthnot and others have done, will be given at length in my edition of
Celsus; meantime the reader may consult Pliny XXI. 34.

† Not eight drachms, as the common ounce. The excellent Arbuthnot,
(284) was misled by not comparing the above passage of Pliny with Celsus.
V. 18, 1.

in Dr. Duncan's table; for this being the mode in which I have generally assumed the medium of the ancients, I thought it fair to compare it with the modern medium obtained in the same manner; besides the advantage of having the authority of a physician and author so justly celebrated in this department. It produces no other difference in the general result than that of making the disproportion between the ancient and modern doses too small. As Dioscorides appeared the most authentic and judicious evidence, his doses merited the first place; his numbers, besides, are often written in compound Greek words, so as to preclude all possibility of mistake by the transcriber. But, on proceeding to examine the other authors who have given the doses of violent medicines, we shall find the coincidence appear so perfect as to remove any shadow of doubt that might still remain respecting the single testimony of Dioscorides.

The most perspicuous and precise author of all antiquity is Celsus; and being engaged at the time of the enquiry in preparing an edition of that author, I set about examining the few doses which he has delivered.

1. The famous antidote of Mithridates consisted of thirty-five different ingredients, mostly aromatics and bitters combined with opium, so that 10743 gr. contained $248\frac{1}{3}$ of opium, or $\frac{1}{43}$. Celsus (179) gave this as an anodyne to the size of an Egyptian bean, whose dimensions, indeed, are not well known, but which, as Dioscorides informs us, was larger than a common bean. Rhodius, ad Comp. Larg. 95, makes it of the same weight as a victoriatum, or two scruples; but he makes the common bean a drachm, *Ad Scrib.* 13, and, therefore, we must consider the Egyptian bean as heavier than a drachm. But as an anodyne it was given to the bulk of an almond, or four times the size of the former, in which case it would contain about six grains; for $\frac{62}{43} = 1\frac{1}{2}$, and $1\frac{1}{2} \times 4 = 6$ grains nearly, which, instead of being an antidote for poison, might, in modern times, itself prove fatal; at any rate, the party who previously swallowed such a dose must have made a poor figure at those splendid but treacherous entertainments of the ancient

great, where death so often lurked in the cup which the Dii Geniales had in vain consecrated to friendship and festivity.*

2. The cough mixture of Athenio, 248 grains to 62 of opium, dose, an Italian bean. Now, $\frac{248}{62} = \frac{4}{1}$. Call the bean even ϑ ij. or 40 gr. and $\frac{40}{4} = 10$ grains of opium in the dose.

3. Another cough mixture had opium $\frac{248}{62}$, or one in four parts as before, and the same dose, or rather its double, being two catapotia of the size of a common bean; therefore, = 20 grains.

4. A composition for Ischuria had opium $\frac{4298}{434} = 1$ part in 9.9 or 10; but $\frac{62}{10} = 6\frac{1}{5}$ grains of opium for a dose; for this is what was contained in the Egyptian bean.

Throw these together as above, and we have

Table of Doses from Celsus.

Mithridate, a dose of	6 gr.	$\frac{6}{4} = 1\frac{1}{2}$
Athenio's cough-mixture,	10	$\frac{10}{4} = 2\frac{1}{2}$
Another do.	10	$\frac{10}{4} = 2\frac{1}{2}$
A mixture for ischuria,	$6\frac{1}{5}$	$\frac{6\frac{1}{5}}{4} = 1\frac{3}{4}$

We see by the above table, that when the enormous doses of Celsus are divided by 4, which is the quotient of the mean ancient doses by the mean modern,† that they then assume a rational magnitude, and, indeed, such as would be given at the present time on his indications. Yet I am not inclined to insist much on the force of this coincidence, as the evidence from other authors is much more direct.

Hippocrates, like the rest, rarely mentions doses; but when he does, it is exactly in the same excessive ratio. He gives an obolus, or $10\frac{1}{2}$ grains of elaterium, in a female case.‡ His other very few doses specified are not of substances that afford any decisive information to our inquiry.

* "When thou sittest to eat with a ruler, consider diligently what is before thee; and put a knife to thy throat if thou be a man given to appetite. Be not desirous of his dainties, for they are deceitful meat."—Prov. XXIII. 1, 2, 3.

† P. 351, supra.

‡ De Superfotatione.

Scribonius Largus gives a catapotium for a cough with expectoration, in which the opium makes $\frac{1}{2} \frac{37}{16}$, or $\frac{1}{2}$ of the whole composition, yet he gives three or four pills of the size of a vetch pea in the course of the night. Say the pill weighed only 2 gr., yet 2×3 or 4 , will give 6 to 8 grains of opium for one night's dose.

The same author gives another catapotium for an old cough, which has $\frac{62}{137}$ of opium, or $\frac{1}{2}$, and this is likewise given three or four times a night to the size of a bean, amounting to nearly a scruple of opium!

His cough pastilli, or lozenges, weigh 31 grains, of which the $\frac{1}{7\frac{1}{2}}$ is opium, or each lozenge contains four grains. For Ischuria he gives a drachm, or 62 grains of a medicine which has $\frac{62}{11}$ or $5\frac{1}{2}$ grains of opium in it.

Marcellus lived under the emperors Gratian and Theodosius, and is said to have been an empiric. He gives 31 grains of aloes for a laxative, and this in many formulæ.

Of a purging medicine, $\frac{2}{3}$ of which consisted of scammony and black hellebore, he gave the size of a hazle-nut, probably a drachm.

He gave troches weighing $\mathfrak{z}\text{i}$. in nephritic pains, and the $\frac{1}{6}$ of these was opium; hence $\frac{62}{6} = 10\frac{1}{3}$ grains of opium in each dose.

Rufus Ephesius orders pulp of colocynth for a purge $\mathfrak{z}\text{i}$. or 62 grains. Aloes $\mathfrak{z}\text{ij}$. or 124 grains of the juice of tithymallus (very acrid) $\mathfrak{z}\text{i}$. = 62 grains. Paulus Aegineta gave for purging medicines the following doses: of Aloes $\mathfrak{z}\text{i}$. = 62 grains; of black hellebore, $\mathfrak{z}\text{i}$. = 62 grains; of scammony, 4 oboli, or 42 grains; of colocynth, $\mathfrak{z}\text{i}$. = 62 grains; of elaterium, 3 oboli, or 31 grains; of the oxide of copper, 31 grains.

The same author gives compound purgatives, with doses agreeable to the simple ones just given. Thus the famous "Purgatoria ex Hermodactylo Podagrica," or arthritic purgative of colchicum, now ascertained* to be identical with the "Eau Medicinale d'Husson," is given by him as follows:

* Vide Medico-Chirurgical Transactions.

"PURGATORIA EX HERMODACTYLO PODAGRICA."

"Hermodactyli quadrantem (= $\frac{3}{4}$ iij.

Anisi, Cumini Æthiopici, ameos, thymi corymborum, piperis albi, zingiberis, singulorum $\frac{3}{4}$ iij., epithymi $\frac{3}{4}$ ss. Dosis $\frac{3}{4}$ iv.—aliqui sex dant. Dantur manè cum condito, aut aqua mulsa, aut mero fervefacto."

On calculation, it easily appears that one-third of the composition is colchicum root; and the less daring practitioners, therefore, who ordered $\frac{3}{4}$ iv. of this medicine were giving $\frac{81}{3}$, or 27 grains of colchicum root to their patient.

Aretæus seems to give similar doses, but, like the rest, mentions very few. To bring the whole of these observations into one view, we shall throw them into a tabular form.

Tabular view of Ancient Doses continued.

I. HIPPOCRATES.

Elaterium, in dose of	$10\frac{1}{2}$ gr.	but $\frac{10\frac{1}{2}}{4} = 2\frac{1}{2}$ gr.
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II. SCRIBONIUS LARGUS.

Cough pill, having of opium	6 to 8 gr.	$\frac{6-8}{4-4} = 1\frac{1}{2}$, 2 gr.
Old cough-pill, do.	20	$\frac{20}{4} = 5$
Cough-lozenge, do.	$5\frac{1}{2}$	$\frac{5\frac{1}{2}}{4} = 1\frac{1}{2}$
Medicine for ischuria, do.	$5\frac{1}{2}$	$\frac{5\frac{1}{2}}{4} = 1\frac{1}{2}$

III. MARCELLUS.

Aloes for a laxative,	31 gr.	$\frac{31}{4} = 8$ gr.
in many formulæ,		
Purge of Scammony & Hellebore,	31	$\frac{31}{4} = 8$
Nephritic Troches, having opium	$10\frac{1}{2}$	$\frac{10\frac{1}{2}}{4} = 2\frac{1}{2}$

IV. RUFUS EPHESIUS.

Purge of Colocynth, pulp.	62 gr.	$\frac{62}{4} = 15$ gr.
Aloes,	124	$\frac{124}{4} = 31$
Juice of Spurge	62	$\frac{62}{4} = 15$

V. PAULUS AEGINETA.

Purge of Aloes,	62 gr.	$\frac{62}{4} = 15 \text{ gr.}$
Black Hellebore,	62	$\frac{62}{4} = 15$
Scammony,	42	$\frac{42}{4} = 10\frac{1}{2}$
Colocynth,	62	$\frac{62}{4} = 15$
Elaterium,	31	$\frac{31}{4} = 8$
Oxide of Copper,	31	$\frac{31}{4} = 8$
Colchicum root,	27	$\frac{27}{4} = 6\frac{3}{4}^*$

From all these facts taken together, it appears that the ancients, from Hippocrates downwards, were in the habit of giving doses at least four times stronger than ours; and that this ratio obtains with remarkable uniformity through all their works, embracing a period of time little short of a thousand years. The coincidences are so numerous that no one can ascribe them to chance; indeed, we can easily trace the dose of Dioscorides and the older writers copied into the pages of the latest, as of Paulus and Marcellus. Let it not be imagined that these extraordinary doses are culled from amongst a great many where the quantity was moderate. I have nowhere done so, but have always noticed their ordinary doses when the ancients themselves mention them as such. Where, on comparison with the ancient authors, the learned reader may form a different opinion, I would beg him to consider that we, at this time, can only judge of their mode of prescribing by what I call the *impossible* doses, just as we arrive at truth in other sciences by the "*reductio ad absurdum*." It is on this mode of reasoning, and the discovery of a secret quadruple subdivision of doses, that the interest of the present inquiry, if it has any, must rest; for the labour of turning over ancient books, and

* I have not taken the trouble to calculate Dr. Duncan's arithmetical mean corresponding to these doses, but the reader can easily do this for himself.

comparing examples, is a drudgery too trite and mechanical to deserve much attention from the readers of this studious, intellectual age.* He must remember that no judicious physician of the present day prescribes an active medicine to its utmost dose in compositions wherein it is only of collateral benefit; and our posterity would judge very much amiss of our ordinary doses of opium, if they were to deduce them from the dose of the London paregoric, or the electuarius catechu compositum of the Edinburgh pharmacopœia. It follows, that occasional moderate doses, did they even occur more frequently in ancient authors, could not decide the matter at issue.

On what principle, it may be asked, are these extravagant, nay, murderous doses of the most acrid and poisonous matters to be explained? Does there, after all, exist some mistake in the numbers from long continued and careless transcription? or do we entirely mistake the true weight and relation of the Roman denarius† to our drachm troy?—Did the ancient physicians really exhibit these doses to patients who, from early habits of activity and exercise then almost universal, had bodies able to resist their influence? Or, did they in order to conceal the art from the profane vulgar, purposely falsify, and for convenience do this in a certain quadruple ratio?—employing weights graduated indeed like those in common use, but secretly understood to be subject to this fourfold division? Each of these conjectures may seem to merit attention, and I shall close this communication with a few remarks on their individual probability.

I. That the doses above, extracted from the ancient authors, have not been altered by the transcribers, is clearly shewn by the fact, that many of them are not in characters, or numeral abbreviations, but in written words, Greek compounds, such

* Accordingly, wherever it was convenient, I have borrowed freely from the labours of Arbuthnot, just as any one is at liberty to borrow from these.

† The Roman denarius is still extant in abundance; and its relation to the other weights is so precisely described by the ancients, that its weight troy being known, all the others are obtained in grains by simple proportion. See the table given above. Pliny, xxxiv. 5, seems to insinuate that their alloy was one-eighth in his time, though modern chemists maintain that the ancient coins are nearly pure.

as the word *τείοβολος*, for example, which could not, by any conceivable chance, have been substituted for its fourth part or eight grains; yet this is the term by which Dioscorides denotes the dose of squill requisite to purge off the bile. Indeed, had any error crept into the text of the older authors, it would never have been copied into the succeeding writers, who were generally practitioners, and could not mistake the dose of medicines so familiar, and whose mode of preparation is so precisely described.

II. There may still seem grounds for believing that the moderns have not a correct estimate of the weight denarius, and that there is, therefore, no accurate method of ascertaining the absolute relation of the ancient weights to those of our times. But the ancient denarius or drachm, for they are equivalent terms, was a common Roman money, about the weight of three sixpences of our present* coinage, and is still to be met with abundantly amongst the dealers in old coins; and numbers of them exist in various public collections, in a state of high preservation. Mr. Greaves, the famous antiquarian, weighed many hundreds of them in different countries, and found the medium weight of the denarii consulares to be very steadily 62 grains, whilst the attic tetradrachm, corresponding to our crown piece, and which ought to weigh four times 62, or 248 grains, at an average weighs four times 67, or 268 grains, making the drachm 67 grains. The latter, however, is evidently a provincial variation, of which we have abundance of examples in our own country and times;† indeed, the measure of the congius of Vespasian, a standard vessel which is still in existence, agrees so exactly with the former weight of the denarius, 62 grains, that there cannot exist a rational doubt of its exactness. A judicious and learned friend, who has spent a considerable time in Italy, and whose connections and pursuits naturally gave an interest to the inquiry, found the

* 1816.

† The cause of this difference seems to have been the alloy, which is commonly one-twelfth, and which the ancients probably did not consider of any value; and, therefore, only estimated the weight of these coins by the pure gold or silver contained in it. This supposition is simple, and explains the difference exactly.

present drachm used by the apothecaries of Italy, to correspond exactly with the ancient Italian denarius or drachm, ascertained by Greaves, consisting of 62 grains troy. Indeed, it is natural to suppose, that, of all other places, the apothecary's shop must be that where uniformity would be longest and most exactly preserved; deviations there being attended with the most immediate and fatal consequences, whilst the usual motives for change are totally wanting, their goods being generally bought in by a different weight. The same excellent physician conjectures, and with great probability, that, were any one to make the experiment, the above weight of 62 grains troy would still be found in the apothecaries' drachm of modern Greece, unaltered, perhaps, since the days of Aristotle.

III. Did the ancient physicians then, really exhibit the above doses to their patients, who, from early habits of activity and exercise, then almost universal, may be supposed to have had bodies able to resist their influence? This explanation cannot, I fear, be admitted. It is incontestably established that the bodies of the Greeks and Romans, whose physic we are considering, were not larger than those of the present race of mankind; nay, more,—we have the constant unbiassed evidence of their own historians, orators, and poets, that their stature and bodily strength was considerably inferior to that of the Asiatics of the south, or of the Gauls to the north of their country. As it is probable that little change has taken place in the personal strength of the latter, who were true Celtae, that tribe, like the Jews, having a wonderful tendency to retain their primitive habits; so, reasoning upwards, it follows, that the ancient Greeks and Romans must have possessed less personal vigour, and therefore, must have less resisted the action of excessive doses than a modern Welshman or Scotch Highlander, whose *vis medicatrix* would, notwithstanding, be somewhat staggered at a 20 grain dose of opium or sulphate of copper. Much, however, may be said on the other side of the question; and the almost total absence of diseases of debility, of scorbutus, the cutaneous tribes, affections of the heart, of typhus fever, of scrofula, of rickets, of chronic rheumatism, in the medical works of the ancients, would seem to

savour something of modern degeneracy.* Their lives were much more debauched than ours, and had their constitutions been as weak, the same consequences must have been observed. Indeed, their general practice of vomiting in order to sharpen the appetite must have given such a shock to the system as few of our modern gourmands would like to undergo for the enjoyment in prospective; yet this practice had prevailed from the most early times, since Hippocrates alludes to it, but thinks it wiser to vomit but once a fortnight. The instances of debauch given by their authors far exceed the eating and drinking matches of the English newspapers,—nay, many of them were habitual. Thus Cato allowed his slaves individually, during the Saturnalia, seven pints, or four bottles of wine, per diem, which would appear in this age rather a liberal allowance. Cicero's son was called Bicongius, because he was accustomed to drink two congii off at a draught. But two congii are seven quarts or eight bottles of wine! the wine may have been weak, but who of these times could have drank off as much water with impunity? Pliny and others abound in such examples, many of them much more wonderful, the present being selected merely for the eminence of the individual and the impossibility of mistake in respect of quantity. Lastly, we find the most execrable instances of corporeal and mental depravity among the ancients enjoying uninterrupted health. The infamous tyrant and debauchee, Nero, was only three times sick in fourteen years. “*Nam qui luxuriæ immoderatissimæ esset, ter omnino per XIV. annos languit; atque ita, ut neque vino, neque consuetudine reliqua abstineret;†*”—and the like is reported of many others. Even the satirists, whose style was exaggeration, and whose proper subjects were vice and luxury, never mention other consequences than *crapula* or indigestion, from the disgusting gluttony of the times; very rarely even *gout*. Notwithstanding all this, however, we must incline to the former opinion, that the ancients were less robust than we are; it being now well ascertained,‡ that of all the two legged varieties of the creation, whether wild or tame,

* This is a difficult passage. Should it not be—would seem to savour of something different from modern degeneracy.

† Suet. de Nerone.

‡ See Lawrence's *Physiology, Zoology, and History of Man*.

an Englishman possesses the highest muscular power; and hence, *a fortiori*, stronger than the Celts, who were, and are, as we have just seen, stronger than the ancient Romans.

IV. The only remaining mode in which this difficulty can be solved is, by supposing that, in order to conceal the art from the profane vulgar, the physicians of antiquity intentionally falsified their doses; and, for convenience, did it always in a certain uniformly quadruple ratio; employing weights, indeed, graduated like those in common use, but secretly understood to be subject to this fourfold division. We arrive at this inference by the method of exclusion, for we have seen above, that every other conceivable mode of accounting for the excessive doses of ancient medicine is without foundation. In supposing that this was the case, we do no more injury to the morality of the ancients, than to the moderns, in allowing that the practice of prescribing in Latin words and Roman characters is for the same purpose;* a thing which few deny, and many writers openly avow. That they *concealed* their doses is manifest from their writings, and from the singular fact, that though we have seen above that they had choice of books on posology, not one of these have reached us, either of Greek or Roman composition; being probably attainable to none but practitioners.

The facts are so obvious and striking, that Dr. Arbuthnot, in the year 1727, formed nearly the same conjecture; and if concealment was practised, it is evident it was only by falsifying the weights and measures of the day in a *constant* ratio, that one medical man could become intelligible to another; or, indeed, have a motive for naming the dose at all, which otherwise must have been completely unintelligible to his readers. A secret understanding, then, of a diminution of the dose in a fixed ratio appears necessary; and as the tables given above show plainly enough that this ratio was a quadruple one, I shall not longer insist on it here; but, with renewed request, that some one better qualified will take this curious circumstance into consideration, conclude these few remarks on a subject rather beyond my opportunities of inquiry.

South College Street, Dec. 25, 1819.

* "Scilicet qui non mulierculis aut circumforaneis medicis artem imper-tiar."—Greg. Consp.

An Account of a Membrane in the Eye, now first described.

By ARTHUR JACOB, M. D., Member of the Royal College of Surgeons in Ireland, Demonstrator of Anatomy, and Lecturer on Diseases of the Eye in the University of Dublin.

[From the Philosophical Transactions, for 1819. Part II.]

ANATOMISTS describe the retina as consisting of two portions, the medullary expansion of the nerve, and a membranous or vascular layer. The former externally, next to the choroid coat, and the latter internally, next to the vitreous humour.* All, however, except Albinus and some of his disciples agree, that the nervous layer cannot be separated so as to present the appearance of a distinct membrane, though it may be scraped off, leaving the vascular layer perfect. That the medullary expansion of the optic nerve is supported by a vascular layer, does not, I think, admit of doubt; but it does not appear that Albinus was right in supposing that the nervous layer can be separated in form of a distinct membrane, though shreds of a considerable size may be detached, especially if hardened by acid or spirit.

Exclusive of these two layers, I find that the retina is covered on its external surface by a delicate transparent membrane, united to it by cellular substance and vessels. This structure, not hitherto noticed by anatomists, I first observed in the spring of the last year, and have since so frequently demonstrated, as to leave no doubt on my mind of its existence as a distinct and perfect membrane, apparently of the same nature as that which lines serous cavities. I cannot describe it better, than by detailing the method to be adopted for examining and displaying it. Having procured a human eye, within forty-eight hours after death, a thread should be passed through the layers of the cornea, by which the eye may be secured under water, by attaching it to a piece of wax, previously

* Ruysch. Epist. Anat. Prob. xiii. Albinus, Annot. Acad. lib. iii. cap. xiv. Haller, Elem. Phys. t. v. lib. xvi. sect. 2. Zinn. Descrip. Anat. Oculi, cap. iii. sect. iii. Sabatier, Boyer, Charles Bell, Cuvier, &c.

fastened to the bottom of the vessel, the posterior half of the sclerotic having been first removed. With a pair of dissecting forceps in each hand, the choroid coat should be gently torn open and turned down. If the exposed surface be now carefully examined, an experienced eye may perceive that this is not the appearance usually presented by the retina; instead of the blue-white reticulated surface of that membrane, a uniform villous structure, more or less tinged by the black pigment, presents itself. If the extremity of the ivory handle of a dissecting knife be pushed against this surface, a breach is made in it, and a membrane of great delicacy may be separated and turned down in folds over the choroid coat, presenting the most beautiful specimen of a delicate tissue which the human body affords. If a small opening be made in the membrane, and the blunt end of a probe introduced beneath, it may be separated throughout, without being turned down, remaining loose over the retina; in which state, if a small particle of paper or globule of air be introduced under it, it is raised so as to be seen against the light, and is thus displayed to great advantage; or it is sometimes so strong as to support small globules of quicksilver dropped between it and the retina, which renders its membranous nature still more evident. If a few drops of acid be added to the water after the membrane has been separated, it becomes opaque and much firmer, and may thus be preserved for several days, even without being immersed in spirit.

That it is not the nervous layer I detach, is proved by the most superficial examination; first, because it is impossible to separate that part of the retina so as to present the appearance I mention;* and, secondly, because I leave the retina uninjured, and presenting the appearance described by anatomists; especially the yellow spot of Soemmering, which is never seen to advantage until this membrane be removed: and hence it is that that conformation, as well as the fibrous structure of the retina in some animals, becomes better marked from remaining some time in water, by which the membrane I speak of is detached.

* See Haller, Zinn. &c. loc. cit.

The extent and connexions of this membrane are sufficiently explained by saying, that it covers the retina from the optic nerve to the ciliary processes. To enter into further investigation on this subject, would lead to a discussion respecting the structure of the optic nerve, and the termination of the retina anteriorly, to which it is my intention to return at a future period.

The appearance of this part I find to vary in the different classes of animals and in man, according to age and other circumstances. In the fœtus of nine months it is exceedingly delicate, and with difficulty displayed. In youth it is transparent, and scarcely tinged by the black pigment. In the adult it is firmer, and more deeply stained by the pigment, which sometimes adheres to it so closely as to colour it almost as deeply as the choroid coat itself; and to those who have seen it in this state, it must appear extraordinary that it should not have been before observed. In one subject, aged fifty, it possessed so great a degree of strength as to allow me to pass a probe under it, and thus convey the vitreous humour covered by it and the retina from one side of the basin to the other; and in a younger subject I have seen it partially separated from the retina by an effused fluid. In the sheep, ox, horse, or any other individual of the class *mammalia* which I have had an opportunity of examining, it presents the same character as in man, but is not so much tinged by the black pigment, adheres more firmly to the retina, is more uniform in its structure, and presents a more elegant appearance when turned down over the black choroid coat. In the bird it presents a rich yellow-brown tint, and when raised, the blue retina presents itself beneath: in animals of this class, however, it is difficult to separate it to any extent, though I can detach it in small portions. In fishes, the structure of this membrane is peculiar and curious. It has been already described as the medullary layer of the retina by Haller and Cuvier,* but I think incorrectly, as it does not present any of the characters of nervous structure, and the retina is found perfect beneath it. If the sclerotic coat be removed behind, with the choroid coat

* Element. Phys. tom. v. lib. xvi. sect. ii. Cuvier, Leçons d'Anat. Comp. tom. ii. p. 419.

and gland so called, the black pigment is found resting upon, and attached to, a soft, friable, thick, fleecy structure, which can only be detached in small portions, as it breaks when turned down in large quantity. Or if the cornea and iris be removed anteriorly, and the vitreous humour and lens withdrawn, the retina may be pulled from the membrane, which remains attached to the choroid coat, its inner surface not tinged by the black pigment, but presenting a clear white, not unaptly compared by Haller to snow.

Besides being connected to the retina, I find that the membrane is also attached to the choroid coat, apparently by fine cellular substance and vessels; but its connection with the retina being stronger, it generally remains attached to that membrane, though small portions are sometimes pulled off with the choroid coat. From this fact I think it follows, that the accounts hitherto given of the anatomy of these parts are incorrect. The best anatomists* describe the external surface of the retina as being merely in contact with the choroid coat, as the internal with the vitreous humour, but both totally unconnected by cellular membrane or vessels, and even having a fluid secreted between them: some indeed speak loosely and generally of vessels passing from the choroid to the retina; but obviously not from actual observation, as I believe no one has ever seen vessels passing from the one membrane to the other. My observations lead me to conclude, that wherever the different parts of the eye are in contact, they are connected with each other by cellular substance, and, consequently by vessels; for I consider the failure of injections no proof of the want of vascularity in transparent and delicate parts, though some anatomists lay it down as a criterion. Undoubtedly the connexion between these parts is exceedingly delicate, and hence is destroyed by the common method of examining this organ; but I think it is proved in the following way. I have before me the eye of a sheep killed this day, the cornea secured to a piece of wax fastened under water, and the posterior half of

* See Haller, *Elem. Phys.* tom. v. lib. xvi. sect. ii. Zinn. *cap. ii. sect. i. § ii* Boyer, *Anat.* tom. iv. p. 113. Sabatier, tom. ii. p. 70. Bichat, *Anat. Deser.* tom. ii. p. 447. Cuvier, *Leçons d'Anat. Comp.* tom. ii. p. 418. Charles Bell, *Anat.* vol. iii. p. 51. Ribes, *Mem. de la Med. Soc. d'Emulation*, tom. viii. p. 635.

the sclerotic coat carefully removed. I thrust the point of the blade of a pair of sharp scissars through the choroid coat into the vitreous humour, to the depth of about an eighth of an inch, and divide all, so as to insulate a square portion of each membrane, leaving the edges free, and consequently no connexion except by surface; yet the choroid does not recede from the membrane I describe, the membrane from the retina, nor the retina from the vitreous humour. I take the end of the portion of choroid in the forceps, turn it half down, and pass a pin through the edge, the weight of which is insufficient to pull it from its connexion. I separate the membrane in like manner; but the retina I can scarcely detach from the vitreous humour, so strong is the connexion. The same fact may be ascertained by making a transverse vertical section of the eye, removing the vitreous humour from the posterior segment, and taking the retina in the forceps, pulling it gently from the choroid, when it will appear beyond a doubt that there is a connexion between them.

Let us contrast this account of the matter with the common one. The retina, a membrane of such delicacy, is described as being extended between the vitreous humour and choroid, from the optic nerve to the ciliary processes, being merely laid between them, without any connexion, and the medullary fibres in contact with a coloured mucus retained in its situation by its consistence alone. This account is totally at variance with the general laws of the animal economy: in no instance have we parts, so dissimilar in nature, in actual contact: wherever contact without connexion exists, each surface is covered by a membrane, from which a fluid is secreted; and wherever parts are united, it is by the medium of cellular membrane, of which serous membrane may be considered as a modification. If the retina be merely in contact with the vitreous humour and choroid, we argue from analogy, that a cavity lined by serous membrane exists both on its internal and external surface; but this is not the fact. In the eye a distinction of parts was necessary, but to accomplish this a serous membrane was not required; it is only demanded where great precision in the motion of parts was indispensable, as in the head, thorax, and abdomen; a single membrane, with the interposition of

cellular substance, answers the purpose here. By this explanation we surmount another difficulty: the unphilosophical idea of the colouring matter being laid on the choroid, and retained in its situation by its viscosity, is discarded; as it follows, if this account be correct, that it is secreted into the interstices of fine cellular membrane here, as it is upon the ciliary processes, back of the iris, and pecten, under the conjunctiva, round the cornea, and in the edge of the membrana nictitans and sheath of the optic nerve in many animals. Dissections are recorded where fluids have been found collected between the choroid and retina, by which the structure of the latter membrane was destroyed. The explanation here given is as sufficient to account for the existence of this fluid, as that which attributes it to the increased secretion of a serous membrane.

I take this opportunity of describing the method I adopt for examining and displaying these and other delicate parts; a method which, though simple, will, I expect, prove an important improvement in the means of scrutinizing the structure of animal and vegetable bodies. I procure a hollow sphere of glass from two to three inches in diameter, about one-fourth of which is cut off at the part where it is open, and the edges ground down so as to fit accurately upon a piece of plate glass, the surface of which is also ground: the object to be examined is attached to a piece of wax fastened upon the plate of glass, and immersed in a basin of water, with the cut sphere, which is inverted over it, of course full of water, and the whole withdrawn from the basin. The part may thus be examined under the most favourable circumstances; it floats in water, the only method by which delicate parts can be unfolded and displayed: the globular form of the vessel answers the purpose of a lens of considerable power and perfection, at the same time that it admits light in any quantity or direction to illuminate the object; and, what is of the utmost importance, a preparation of the greatest delicacy may thus be handed round a class in safety.

Persian Opium.

[From Poulson's American Daily Advertiser.]

AN article sold under the name of Persian opium, having been introduced into this city from New York, and sold for opium, which was generally considered as spurious, a number of druggists of this city met at Yohe's Hotel, to take the subject into consideration. A committee was appointed to examine the article, and report to a subsequent meeting. The report having been read, it was ordered that the same should be printed.

The committee appointed to examine the article introduced into some of the drug stores of this city, called Persian Opium, report as follows:—

Of all the articles of the materia medica, opium is the most important, and the most essential to the successful practice of a physician. It is prescribed in a greater range of diseases, than any other medicine, and is an indispensable remedy in many of the most fatal and terrible disorders that afflict the human system. It is not alone to allay the distressing effects of nervous and constitutional irritation, and to alleviate the anguish and torture of pain, that opium is administered; in the crisis of acute disease, life and death are in question, and the result is depending on the judicious administration of a grain of pure opium. It is not necessary here to enlarge on the high obligation imposed on all dealers in drugs to abstain from tampering with the articles they sell, to vend none of whose purity or efficacy they have a doubt; or to dwell on the guilt and criminality of those, who will deliberately adulterate their medicine, or delude the unwary practitioner, by imposing on him a deleterious or inert remedy, to the destruction of his patient.

In the commerce of this country and of Europe, two species of opium are only known, the Turkey and the East India—Persian opium has not before been heard of. The Turkey opium is much more pure than the East India, and is that

which is solely used in the practice of medicine in Europe and America. It is by the strength and qualities of Turkey opium, that practitioners regulate their prescriptions, and it is that consequently, which should alone be sold when opium is to be employed as medicine.

The committee, in order to perform the duty intrusted to them, proceeded to a comparative examination of Turkey opium, and the article called Persian opium, in their sensible, chemical and medical properties; of which they present the ensuing statement.

1st. *Sensible properties*—Persian opium.—Its colour is deep black; soft in consistency, as though lately made up; oily, greasing the paper in which it is enveloped; of a nauseous not well defined smell; of a mawkish taste, with a feeble impression of sweetness. Turkey opium—colour of a reddish brown; when fresh imported, soft, but most generally hard; never oily; of a peculiar virose, well defined smell which cannot be mistaken; of a bitter, strong taste, leaving a sense of pungency and heat on the tongue and fauces.

The sensible properties of these two articles are so entirely distinct, that the most inexperienced person could not hesitate in distinguishing between them. It is the sensible qualities of medicines, that the dealers in them generally consult to ascertain their goodness; and any one with the slightest knowledge of opium, must have suspected the sophistication, or the inertness of Persian opium at the first glance.

2d. *Chemical Properties*.—Half an ounce of Persian opium, so called, was infused in four ounces of water. The whole of it was dissolved, or diffused through the water, leaving no residual matter. When filtered, the infusion was *black*, and there was collected on the filter 37 grains of a black matter, having a faint smell of tobacco.

The same quantity of Turkey opium was infused in the same quantity of water. The soluble parts were soon dissolved, leaving behind an elastic, plastic mass, having a strong resemblance to gluten, and which, when dried, weighed 71 grains. The solution was of a *light wine colour*.

To a portion of the watery solution of Persian opium, was added a small quantity of alcohol. A precipitation of a dirty

white colour, diffused through the solution, immediately took place. The same quantity of the watery solution of Turkey opium, and the same quantity of alcohol, as in the preceding experiment, were mixed together. The solution remained perfectly transparent.

To ascertain the effects of different tests, the watery solution of the spurious and Turkey opiums was used, the result of which is contained in the following experiments:

Infusion of galls, Persian opium.—A slight yellowish white coloured precipitate that did not subside. Turkey opium—a copious white precipitation, that immediately subsided with an appearance of being curdled.

Acetate of lead, Persian opium.—A slight precipitation of a brown colour, diffused through the solution. Turkey opium—white precipitate soon subsiding.

Sulphate of copper, Persian opium—No immediate change, dark coloured precipitate on standing. Turkey opium—instant precipitate of a white colour.

Sulphate of zinc, Persian opium.—No precipitate. Turkey opium—a copious white precipitate.

Subcarbonate of potash, Persian opium.—No precipitation—the solution becoming slightly turbid, of a dark colour. Turkey opium—A copious curdled precipitate, of a white colour.

Sulphate of iron, Persian opium.—No precipitate—the solution became of a deeper colour. Turkey opium—a reddish brown precipitate, which did not subside.

Half an ounce of Persian opium was infused in four ounces of alcohol. There remained 67 grains insoluble residue. A portion was evaporated to dryness, and a soft sweetish extract was obtained without any resin. To the alcoholic solution water was added, a diffused precipitation ensued, which subsided on standing some hours. The addition of water to the alcoholic solution of Turkey opium, occasioned an immediate copious precipitate subsiding at once.

Having made the above general examination of the chemical qualities of the article submitted to our consideration, it was thought proper to ascertain what were the effects produced by it on the system.

3d. *Medical properties.* A grain was taken by one of the committee. The pulse beat 85 strokes in a minute. No perceptible alteration in it took place. About an hour afterwards, a slight nausea was felt. Another of the committee took 50 drops of the tincture without perceiving any effect. Mr. Britt, a student of medicine, took two grains at a dose, in the morning, two hours after breakfast, his pulse beating 80 strokes. In 5 min. 78—10 min. 76—15 min. 76—25 min. 75—30 min. 76—40 min. 74. Two grains more were taken—5 min. pulse 72—15 min. 72—25 min. 72—35 min. 72. The skin remained perfectly cool; there was no sensation of heat, flushing of the face, or other effects produced by a large dose of opium. An hour and a half after taking the dose, a slight nausea was felt, and about half a gill of fluid thrown from the stomach. One of the students at the hospital took half an ounce of the tincture in one dose, without experiencing any stimulant or soporific effect from it.

From the examination to which the committee have submitted the article called Persian opium, they feel a perfect confidence in pronouncing it to be devoid of the properties of opium; and give it as their opinion, that it cannot be relied on to produce the effects of opium; but, that its administration for that purpose, in critical cases, must be attended with the greatest danger to the life of the patient.

SAMUEL JACKSON,
EDWARD LOWBER,
THOMAS WILTBERGER.

Philadelphia, July, 1820.

SELECTED REVIEWS.

Report from the Select Committee on the Doctrine of Contagion in the Plague, pp. 102. Ordered by the House of Commons to be printed, June 14, 1819.

[From the Edinburgh Medical and Surgical Journal, for Jan., 1820.]

OUR readers will probably recollect, that, in the course of the last session of Parliament, Sir John Jackson moved for a committee to inquire into the expediency of abrogating or modifying the restrictions imposed by the quarantine laws; in which motion he was supported by the Right Honourable F. Robinson, President of the Board of Trade; and the reason assigned for this measure by Sir John Jackson was, that the doctrine of contagion had been shaken by evidence which came before a committee during the preceding Parliament, to inquire into the epidemic fever then prevailing in the metropolis. The principal objects of enquiry on the subjects in question were, *first*, Is the plague capable of being communicated from person to person, either by immediate contact with those diseased, or, intermediately, by contact with infected goods? Or, *secondly*, Is it an epidemic depending only on a peculiar state of atmosphere? We shall now give the report in full, which is the result of their labours.

“Your committee being appointed to consider the validity of the received doctrines, concerning the nature of contagious and infectious diseases, as distinguished from other epidemics, have proceeded to examine a number of medical gentlemen, whose practical experience, or general knowledge of the subject, appeared to your committee most likely to furnish the means of acquiring the most satisfactory information. They have also had the evidence of a number of persons, whose residence in infected countries, or whose commercial or official employments, enabled them to communicate information as to

facts, and on the principle and efficacy of the laws of quarantine. All the opinions of the medical men whom your committee have examined, with the exception of two, are in favour of the received doctrine, that the plague is a disease communicable by contact only, and different in that respect from epidemic fever; nor do your committee see any thing in the rest of the evidence they have collected, which would induce them to dissent from that opinion. It appears from some of the evidence, that the extension and virulence of the disorder is considerably modified by atmospheric influence; and a doubt has prevailed, whether, under any circumstances, the disease could be received and propagated in the climate of Britain. No fact whatever has been stated to shew, that any instance of the disorder has occurred, or that it has ever been known to have been brought into the Lazarettos for many years: but your committee do not think themselves warranted to infer from thence, that the disease cannot exist in England; because, in the first place, a disease resembling, in most respects, the plague, is well known to have prevailed here, in many periods of our history, particularly in 1665 and 1666. And further, it appears, that in many places, and in climates of various nature, the plague has prevailed after intervals of very considerable duration. Your committee would also observe, down to the year 1800, regulations were adopted, which must have had the effect of preventing goods, infected with the plague, from being shipped directly for Britain; and they abstain from giving any opinion on the nature and application of the quarantine regulations, as not falling within the scope of inquiry to which they have been directed, but they see no reason to question the validity of the principles on which such regulations appear to have been adopted."

We shall defer our remarks on this report until we have laid before our readers, as clear an abstract of the evidence before the committee as our limits will allow.

Dr. Charles Maclean, who was first examined, states, that he has studied the plague in the Greek Pest-Hospital at Constantinople, in which he resided seventeen days, and that, during his stay there, he himself caught the disease. The inferences supplied by his experiments on plague were sufficient

to confirm him in those conclusions against the existence of contagion, which he had previously deduced in respect to yellow fever, typhus, and epidemic diseases generally.

After this sweeping conclusion, he thus modestly speaks of himself, and of his search after truth:

“If it should be thought that this is but an inadequate experience, I would entreat of the committee to recollect how little the value of experience may be commensurate with its duration; and that the result of experience in the application of false knowledge, is but dexterity in the practice of error, which is as much worse than mere inexperience, as false knowledge is worse than absolute ignorance. Since the establishment of the doctrine of contagion, I am not aware that any other physician, now living, has been experimentally in such intimate collision as myself with what is called the plague of the Levant;* and if the result of my researches should ultimately prove adequate to dispel, in respect to the cause of that malady, the darkness of centuries, they will not be denied to be of unprecedented efficiency.”

He then goes on to assert, that the plague is not contagious, and being asked to explain how he caught the fever when in the Pest-Hospital, he replied, “by the air.”

“It was in August, a month in which plague generally prevails most at Constantinople; I was a stranger to the climate and otherwise under particular circumstances; the deprivation of food, insufficiency of nourishment, and also some degree of irritation of mind, under the idea, that the people around me were endeavouring to frustrate my object; and all these

* We do not exactly understand the force of this assertion. How many physicians may be now living, who have been in as intimate collision with what is called the Plague of the Levant as Dr. Maclean, we do not know; and if none of them placed themselves in such a situation *experimentally*, we know that some did it *voluntarily*, and from the noblest motives that can influence human conduct. During our expedition to Egypt, the present President of the College of Physicians of Edinburgh, three several times, shut himself up in the lazarettos for months together, when the inferior officers to whom the duty was allotted were themselves infected, and rendered incapable of attending the sick. Nor did he escape with impunity, for he was twice affected with the disease, certainly not from the causes to which Dr. Maclean has ascribed his own illness.

might surely be sufficient to account for the occurrence of disease during the pestilential season, without supposing contagion."

Dr. Maclean, in short, considers, that his having been affected with plague, must be attributed to the climate, short commons, and his being put out of humour; and that his having daily and hourly contact with patients labouring under an aggravated form of that disease, and living in the contaminated air of the hospital, had no share in its production.

Dr. Maclean distinguishes contagious from epidemic diseases, by supposing that the former are capable of being communicated by contact, or contiguity, whereas epidemics are produced by such causes as are capable of operating simultaneously upon any portion, or the whole of a community, and that they cannot become contagious. He adds, that contagious diseases occur only once during life; but on being asked if the small pox be not a contagious disease, and whether it has not occurred more than once in the same person, he admits that it is contagious, but does not believe it ever has twice affected the same person; and, were he to see a case of second small pox, he would distrust even the evidence of his own eyes, p. 13.* In defining the plague, he says; its phenomena embrace almost all the symptoms which the living body is capable of exhibiting, few of these, however, occurring at the same time, in the same person. He quotes Fra Louigi di Pavia, who attended a plague hospital for 30 years, and who says, the surest symptoms of plague are the eyes dusky and turbid, fixed and sparkling; the tongue forked, of a whitish colour, inclining to yellow, with the extremity red, which branches out in a number of small ramifications, exceedingly inflamed. He proceeds to describe, from his own experience, the appearance of the countenance:

"The skin and muscles of the face exhibit a tremulous appearance, and with a certain fierceness of the eyes, and sometimes an involuntary motion of one of the eye-lids, as in winking, gives an expression of rather a ludicrous wildness of

* Syphilis also is a contagious disease, and nobody doubts that it can occur frequently during life.

aspect, increased by the efforts of the patient to preserve the composure of his features, as if conscious of the insubordination of his features to preserve their composure."

The appearance of the tongue varies from blackish to brown, white, and now and then flesh-red colour; he has also seen it of a glossy grey, *resembling the first formation of icicles upon water*. And this last state he considers as promising the most favourable termination. The brain and circulating system are variously affected in the earlier and milder stages of the plague. There is a rapid succession of ideas, hurried speech, a tremulous and unsteady walk, and a distracted look. The higher degrees are accompanied by delirium, anxiety, languor, and melancholy, as well as great muscular debility, throughout the disease. At the commencement, the bowels are usually constipated; but, in the progress of the disease, diarrhœa and hæmorrhage are not unfrequent symptoms; and when severe, denote much danger. Dr. M. adds, however, that none of those symptoms are pathognomic, nor does he consider them as very distinct from those of the severe form of typhus and yellow fever in other countries. He does not put much dependence on the glandular affections as diagnostic of plague, but thinks the carbuncles are more decided marks of the disease, for buboes, he asserts, occur in typhus.* If these observations are correct, the inference, we think, must be, either that every fever is plague, or that nothing is plague.

During the prevalence of the plague at Constantinople, the Frank merchants shut themselves up in their houses, and, by so doing, they generally avoid the disease. But Dr. Maclean attributes the success of this measure to their avoiding vicissitudes of atmosphere, by keeping their windows shut during the pestilential season. Speaking of the experiments of Dr. White, he says, that gentleman first rubbed on his arm matter which was taken from a bubo of a person supposed to have the plague, by which he was not affected; he afterwards inoculated himself thrice in the arm with similar matter: that he was not affected by the two first inoculations, but, after the

* Buboes occur *very rarely* in typhus, and only as sequelæ of the disease; they occur *very commonly* in plague, and always as a primary symptom.

third, he sickened of the plague, and died: which circumstance, nevertheless, Dr. Maclean does not attribute to the experiments previously made by Dr. White, but considers as an accidental coincidence his being seized with the disease at that time.

On being asked his opinion on the utility of the quarantine regulations, he replies, that he does not consider the establishment of lazarettos of the smallest use, and he goes on to say,

“ If it be true, that there never has arrived at any one period of time any one person from the Levant, or any other place, actually labouring under the plague; and if it be true, according to the advocates for contagion, that goods, wares, and merchandize, can retain infection for seven, fourteen, or twenty years; it must be apparent, that with respect to goods, as well as with respect to persons, a quarantine of forty days can be of no sort of use.”

He states, on the authority of Dr. Carunhanha, and of Dr. Grieves, superintendent of quarantine at Malta, that no case of plague had occurred in the lazaretto for fifteen years; nor, in the plague of 1813, was any officer of the lazaretto affected by the disease. On being asked, if he was aware, that, in the towns of the East, the Turks suffer in a greater proportion than the Christian population? he answers, that he knows the fact to have been so represented by Christian travellers in the Levant; but that he considers this as a mere assumption; such travellers cannot possibly know any thing at all of the matter, except what is represented to them by Christians believing in contagion, or by the superintendents of plague establishments; and that the want of bills of mortality amongst the Turks must render such inferences of no value. Yet, in his very next answer, he asserts, that the Turks, who have no belief in contagion, recover in much greater numbers from the plague than the Christians who entertain that belief; and this, he adds, is “ confirmed by facts, and it is stated by various travellers.”

From the above statement, it is sufficiently evident, that Dr. Maclean asserts or denies, as he may find most convenient; for here he has first denied, and then believed, the statements

of travellers; first stated the want of bills of mortality to be an obstacle in ascertaining the proportion of recoveries, and then has done it quite as well without them. The doctrine of contagion, according to Dr. Maclean, originated in the council of Trent: the pope, desirous of removing the council to Bologna, having suborned Fracastorius and certain other physicians to circulate the report, that a malady had broken out, which spread itself rapidly from person to person by contact: and that the ancients had no idea of contagion. We have the following reasons given for believing that the plague is not contagious:

“ 1st, That this and all other epidemic diseases appear generally at certain seasons, and disappear at other certain periods, different in different countries. 2dly, They also cease generally at the time at which the greatest number of persons are affected, which seems wholly incompatible with the existence of contagion. And, 3d, Because they are capable of affecting the same person repeatedly, which there is no proof that contagious general diseases are capable of doing.”

The epidemic season at Smyrna, as stated by Dr. Maclean, is from March to June or July; at Constantinople, it begins in July or August, and ends in November or December;—and, if this be correct, we have thus two places, not far apart, the epidemic seasons of which, taken together, occupy ten months in the year. But it will be found, in the progress of this subject, that Constantinople is never totally free from plague.

This is the substance of Dr. Maclean's evidence, which we have been tempted to give thus much at large, because he is the Goliath of the non-contagionists; but, to avoid going over the same ground again, we shall reserve our remarks until we have gone through the remainder of the evidence.

The next person examined was Dr. Thomas Forster, whose answers were to the following effect:—That he has not any personal knowledge of the plague; but from such information as he has been able to collect, he considers it a contagious disease, rendered less so by ventilation; he believes buboes and carbuncles to be the best diagnostics of this malady; he thinks the difference of our climate the principal cause of our exemption from plague, but does not regard our long immunity from

plague as sufficient security against its recurrence if imported, since there might exist a state of climate and other causes to favour its production; he conceives that epidemics depend much upon a peculiar electric state of atmosphere. With regard to the possibility of an epidemic attacking the same person more than once, he says, that he is acquainted with a person who has had small pox three times; he thinks Lucretius's opinions of the contagious nature of certain diseases as deserving to be noticed on the minutes of committee; and these we shall give presently in some extracts from the sixth book of his poem, *De Rerum Natura*.

Dr. James Johnson had served four or five years in the Mediterranean, where, although he had not an opportunity of seeing the plague, yet he acquired information respecting its nature. He is of opinion that it is contagious, and that its activity depends much on a particular state of the atmosphere; that infected goods or merchandize may communicate the disease, but more particularly the clothes of diseased persons. He entertains this opinion on the authority of Dr. Russell, and other authors who have described the disease; and the circumstance of its being an eruptive disease, strengthens that belief, as other diseases of that character universally arise from specific contagions or poisons. The malignant fevers of Trincomalee, Batavia, and Diamond Harbour, are not analogous to plague. *Epidemic diseases* may at first be produced by atmospheric influence, and afterwards be propagated by contagion. He thinks the return of plague is a revival of infection that has been latent or dormant, until a particular state of atmosphere rouses it to action. Dr. Johnson well remarks, that the non-occurrence of plague in a lazaretto, for fifty or an hundred years, is not a sufficient reason for doing away the quarantine laws; for although the climate of this country may be unfavourable to the production of plague, yet we do not know what share quarantine regulations may have had in our exemption from that disease.

Dr. William Gladstone, when at Constantinople in 1806-7, saw cases of the plague in the months of December, January, February and March. The origin of the disease he takes to be a diseased constitution of atmosphere, and deficient ventila-

tion of the houses; and he attributes the plague of London in 1665 to similar causes. The absence of plague in this country he thinks cannot be attributed to our quarantine regulations, as they are very imperfect; and he does not suppose the contagion of plague ever to have been imported into the lazarettos, as there is not any instance of the expurgators having been affected. He adds, that epidemics may certainly become contagious.

Dr. A. B. Granville, who had seen the plague in Turkey, Greece, Asia, Syria, Egypt, &c. and in Constantinople, where he resided two years, describes the symptoms to be pretty generally these: Dizziness, great pain in the head, great prostration of strength, the nervous system very particularly affected, occasionally sickness of the stomach, and, invariably, the appearance of glandular swellings, with carbuncles, and other local or cutaneous symptoms, particularly livid spots on the body, if the disease goes beyond sixty hours. There are no symptoms of inflammation whatever, none, at least, such as attend inflammatory diseases during their first attack. He is ignorant of the cause, and only knows that the disease exists, and is spread by contagion. In Constantinople, Smyrna, Egypt, and Syria, the Franks, who can afford it, shut themselves up during the plague, and he never heard an instance of such persons being infected where the seclusion was perfect. Of course, this separation could not avail, if the disease was communicated by the air. In Egypt, this separation is continued until St. John's day, June 24th, when the dew falls. Dr. G. considers that the state of the air has no effect in checking the disease, except, perhaps, that it lessens the liability of persons to be affected by it. He understands that the plague has been carried from Aleppo, by caravans, to Bagdat, and Damascus: to the latter place it was carried by the army of some Pashwa, who had been on the coast to assist in the reduction of Jean d'Acre; that it has likewise been carried over land westward from Constantinople to Adrianople.

Dr. Granville distinguishes contagion from infection, confining the former term to the communication of disease, by mere contact with a diseased person, or with something that has been in contact with him, whilst he limits the latter to re-

ceiving the disease through a contaminated atmosphere in a sick chamber. In support of this distinction, he mentions the following occurrence, which we shall give in his own words, as it bears directly on the object of the committee, and is very remarkable. "There is one instance in point amongst the most recent, and it rests on the highest authority." During the plague at Cortu, in 1815,

"One of the villages, which had been infected several months, had for some time, I believe 48 days, exhibited no sign of the plague, owing to the measure of segregation adopted by Sir Thomas Maitland. The village was reported to be released, and fumigation preparatory to its receiving pratique ordered. The officers who had the surveillance of the village during the three or four months, had resided in the church, from there being no house that was not thought to be infected, in which church the priest and the people had been crowded just before the laws of segregation were ordered by Sir Thomas Maitland. Some of these died subsequently, for the church was ordered to be shut the instant the plague began. It was therefore necessary to purify this edifice before the people could go in again, and leave being granted for this purpose, the priest went in, and having touched the cloth of the great altar, so as to purify it by shaking it, he was seized with the plague, beginning with the headache, of such violence, that he fell on the steps of the altar, almost immediately; and in three hours, before he could be carried to the lazaretto, he expired, with buboes under the arm, and livid spots over the body."

Dr. Granville expresses his belief, that the poison, in this instance, was conveyed by touch alone, since the officers who had resided two months in this church were not affected. Since 1814, he says cases have occurred in the lazaretto at Leghorn, where persons touching the infected part of bales of merchandize have been seized with plague. At Marseilles, a similar circumstance has happened twice in fifteen years, and, recently, according to the dispatches of Mr. Hopner, the British Consul at Venice, in October 1818. During the prevalence of plague in the Levant, goods in general are not allowed to be shipped for England, and many vessels perform quarantine at ports in

the Mediterranean, which lessens the probability of the contagion of plague having ever been imported into our lazarettos: But the fact of the plague never having taken place here, for 154 years, or if for six or seven centuries, would not give the hope that it *cannot* exist in a British atmosphere, since we know it existed here in 1665.

John Green, Esq. is treasurer to the Levant Company, and resided in Constantinople from 1774 to 1780. In the year 1778, he witnessed a violent plague during the months of May, June and July, which destroyed 200,000 persons. The most important part of his testimony, is that which expresses his disbelief, that goods cannot convey the contagion of plague; in support of which opinion, he mentions, that the clothes and bedding of persons dying of plague are commonly sold, and used without fear.

He states, that the disease may attack the same person frequently; that he knew an Abbé, of the French hospital, who acknowledged he had had it ten or twelve times. He has heard, however, that there is a particular kind of bubo called "*the Blessed*," which, when it occurs, renders the person much less liable to have the plague again, or if he should be afterwards infected, the disease is slight. Mr. Green conceives, that the dew which attends the hottest seasons has a powerful influence in checking the plague; and five years ago, Mr. Morier informed him, that the plague which had begun to be prevalent, we believe, at Alexandria, ceased suddenly on the coming on of dense fogs, which fogs prevailed also in this country at the same time. The inhabitants of Constantinople believe, that exposing infected clothes for one night to the dew, has more effect in purification, than a similar exposure for a whole week in the sun. This last part, however, shows that apprehension does exist as to danger from this source. He would not recommend the abolition of quarantine laws, as the state of health of crews ought to be inquired into; and if the quarantine establishments of this country were abolished, no matter why, it might occasion the exclusion of our vessels from other countries.

"The lazarettos of England are old men of war, with houses built upon them like an ark. The sides of these houses

are open like a brewhouse, with shutters, and the floors are all open gratings in fact, so that the ventilation is excessive on board these vessels in Standgate creek, greater than it is possible to give on any building on shore. The ships also swing with the tide; that is, when the tide turns, they change their sides to windward every six hours."

The mode of ventilation is different for different kinds of goods. Some articles, as goat's wool, if brought in a ship with a foul bill of health, are ordered to be entirely turned out of the packages, and exposed to the air. Bale goods are slit open on one side, and exposed for some time, which is then sewed up, and the other side exposed. The expurgators are ordered to thrust their arms into the bales as far as possible.

In Dr. M'Leod's evidence, nothing important is added to what we have already stated.

Sir A. B. Faulkner was staff physician to the forces at Malta, during the plague which occurred there in 1813. He considers that this disease is propagated by contact only, or close approximation to the person or thing infected. He gives numerous and decided proofs of the success which attends complete separation of the healthy from the diseased, and instances the public general hospital, public prison, one of the barracks, and all the convents, except one, and the introduction of the disease into this last could be accounted for. He did not perceive that changes of temperature had any effect upon the progress of the disease, but he observed, that a high wind, from whatever quarter it blew, was always accompanied with an increase of the numbers affected. The disease had certainly no connection with marsh miasmata, the part of the town the most subject to marsh fevers, being less affected by plague than many others which were considered more healthy. In the treatment of the plague, camphor, calomel, and topical blood-letting by leeches, were tried, but the remedies found to be of the most service, were cold affusion and turpentine.

Dr. Pym has no doubt that the plague is contagious, but not very actively so. He considers the fever which prevailed at Gibraltar in 1804, and which destroyed 6000 persons, out of a population of 20,000, to have been a more formidable disease than the plague. He thinks it probable, that the contagion of

plague has never been brought into our lazarettos, which he thinks may be accounted for, upon the supposition that the cargoes are packed by healthy persons, or at a time when plague does not prevail. Before 1813, however, the duties of the expurgators were negligently performed.

Sir James M'Grigor was at the head of the medical staff of the British army which came from India up the Red sea, and disembarked at Kossier in May 1801. This army was 7886 strong, of which 3759 were Europeans, and 4127 natives of India; there died of the plague 38 Europeans, and 127 natives of India, in all 165. The disease came on with fever, and buboes; the two first cases could not be closely traced, but the sufferers had been at Rosetta, where there were cases of plague; from these two, four more caught the disease, and all the six died. The disorder is contagious, but he has known instances of contact without any ill effect. There were many instances of its spreading by contagion, particularly one of a prisoner confined in the guard-house, who afterwards appeared to have the plague, which was thus communicated to the other prisoners, and the men on guard. The plague did not appear connected with miasmata, as it was met with in the progress of the army through Upper Egypt. And Sir James M'Grigor thinks it probable, that it always exists in some degree in Egypt.

Dr. James Curry considers the contagious nature of plague to be clearly shown by the order of its progress, and the protection afforded by segregation. He supposes it may be often generated anew; but that it requires for its existence a peculiar state of atmosphere, which he thinks greatly to depend upon electric influence. He believes it was first introduced into the Mediterranean by fomites brought from Jerusalem and Syria by the Crusades.

Dr. Tainsh, surgeon of the *Theseus*, was in the Mediterranean in 1798, 9. He received on board his ship five cases of plague, some of them severe. One of them, that of a Frenchman, had suppurating buboes, but recovered. These cases have been alluded to by Dr. Pym. The disease appears to have been communicated by intercourse between two vessels at sea;

but the circumstances were not inquired into by the committee, and the case is, therefore, rendered of no value.

Mr. Edward Hayes, who is, we believe, a merchant, resided 44 years in Smyrna; during which time he frequently saw the plague. He speaks confidently of the security afforded by confinement within doors during its prevalence. At Smyrna, the disease generally begins in March or April, and ends in August at the latest; but great cold or heat commonly arrest it. He informs us, that goods are shipped at all times, whether the plague prevails or not; that the merchandize may, therefore, be infected; but that the time occupied by the voyage, and the state of the persons exposed to the contagion, are probably causes which prevent its taking effect in this country and many other places; that, abroad, the plague does at times affect the Lazarettos. The quarantine regulations in Holland do not permit any vessel from the Levant to enter, if the plague prevailed there at the time of her departure. He affirms, that plague is communicable by goods; all woollen and linen goods may infect; animal substances, particularly goat's wool, are more dangerous than vegetable. He has known the disease communicated by inoculation, but is not aware that vaccination has been tried as a security against plague. He adds, that the Turks have lately learnt the danger of preserving the clothes of persons dying of this disease.

The evidence of Sir Robert Wilson is only a repetition of what has been already stated by Sir James M'Grigor. Dr. Nevinson, Dr. Powel, Dr. Frampton, and Dr. Ash, profess not to have seen any cases of the plague, but they all agree in believing that it is contagious, both as regards persons and merchandize. The evidence of Dr. Latham is to the same effect; and, in the examination of this gentleman, a strong leaning of the examiners to one side of the question, which we think has been rather injurious to the whole of this investigation, is more than usually conspicuous.

Dr. Frank saw frequent cases of plague, whilst with Sir Ralph Abercrombie, in the year 1800, at Aboukir. He could distinctly trace its progress, by contagion, from one person to the sick and hospital attendants, who afterwards became infected; whilst none of the wounded, who were in a separate

hospital, about half a mile from the former, had the disease. He has seen several instances of contact with impunity, and has himself handled plague patients. His opinion is, that the plague of London in 1665 was imported, and that circumstances might arise in this country favourable to its existing again.

Dr. Harrison has not any doubt of the highly contagious nature of plague; he believes it to be typhus, with the ordinary symptoms more severe, and with the addition of broad purple spots, buboes, and carbuncles. He was at Naples whilst the plague raged, in 1816, at Noya, 150 miles from thence; and, in 1817, there was a contagious and very mortal fever at Naples, supposed to be the plague. Dr. Harrison is of opinion that the quarantine laws are useful, but that they might be modified as to persons; and he mentions a fact, which is very important,—that while passengers, who have made a long voyage, are liable to perform quarantine, couriers, who come in the least possible time, are not under such restrictions. The next person examined was Mr. John Jenkins, a pilot of the port of Liverpool, who says, that, in eleven years, he never knew any one ill on board of the lazaretto, and that there is a surgeon who attends every day.

We come, lastly, to Dr. John Mitchell, whose opinions respecting contagion are precisely those of Dr. Maclean; for which reason, and because a large part of his evidence is founded more upon the opinions of others than his own observation, our abstract of his testimony will be short. He tells us, that Desgenettes and Larrey, the chief physician and surgeon of the French army, exposed themselves to the plague, without taking the disease; and Desgenettes showed Assalini two punctures which he had made on himself, without effect, with a lancet dipped in the pus of a plague bubo. M. Assalini was told by Citizen Martin, captain of the Lazaretto at Marseilles, who for thirty years held that post; that, during that time, he had seen emptied some millions of bales of cotton, silk, fur, feathers, and other goods, coming from several places where the plague raged, without having seen any accident therefrom to the persons employed. Dr. Mitchell classes the plague with malignant fevers, and mentions that Assalini does the same. Dr. Mitchell

lays much stress upon his experience when resident clerk in the Infirmary of this place; from which he concluded that the fever he then saw was not contagious. His opinion, promulgated in his inaugural dissertation, was at that time almost universally considered as heterodox. At any rate, it is completely overthrown by the experience of the late epidemic; during which a succession of matrons, nurses, and resident clerks, in both hospitals, have been infected, with scarcely an exception.

Dr. Maclean comes again before the committee, and this second testimony concludes their volume. He amends his definitions of epidemics, contagion, and infection, and adds fourteen reasons more to those already given for his want of faith in the doctrine of contagion. But we have neither room nor inclination to go deeper into the discussion of his views.

We have now furnished our readers with the substance of the examinations before the Committee of the House of Commons; and that our abstract is not more clear, has arisen from the very desultory manner in which the inquiry was conducted. The number of medical men examined was nineteen; only two of whom, Dr. Maclean and Dr. Mitchell, deny the contagious nature of the plague. The non-contagionists are, consequently, to the contagionists as ten to one. But we feel confident that, in point of fact, the disproportion generally is much greater; and if the committee had extended their examination to the whole of the faculty of London, it is our conviction they could not have raised more than one recruit for Dr. Maclean, perhaps not one; for we do not believe that Dr. Robarts, who, it appears, first made infidels of the right honourable President of the Board of Trade, and the honourable Chairman Sir John Jackson, is at all prepared to go the same length with Drs. Maclean and Mitchell in this heresy.

That the plague is highly contagious is, perhaps, the only inference that can be much relied upon, which results from the labours of the committee. The conflicting assertions as to facts, and the various opinions deduced from similar facts, are, at first sight, not calculated to leave a favourable impression as to the value of medical science; but the numberless sources of error which perplex such investigations, are only known to those

who have long applied to them. For our opinion on the general question, Whether typhus and other fevers are contagious? we shall refer to former numbers of our Journal.* That contagion was a lamentable feature in the epidemic fevers which have recently afflicted Britain and Ireland, is so evident, that we cannot comprehend the organization of that man's mind who doubts, and still less his who denies it.

With regard to the frequent allusions, in the evidence which we have just examined, to the opinions of the ancients on contagion, we do not think them of much value; for it is not sufficient to know that they believed in contagion, unless we are in possession of their reasons, which we cannot know while we are ignorant of the diseases they describe: For example, Whether the plague of Athens, described by Thucydides, was the disease now called plague, is very doubtful; we think it more probable the plague of the present day was then unknown, as Herodotus, who was the contemporary of Thucydides, makes no mention of the plague when describing Egypt, though he has said much of the climate; and mentions, that, at particular seasons, the Egyptians were habituated to taking emetics and cathartics for three successive days, in each of the months they considered the most unhealthy; and it is not to be supposed that so accurate an historian could omit so remarkable a feature as the plague has since formed in the natural history of that country. Lucretius, who wrote about 350 years later, and who was in those days what Armstrong the poet has been in ours, has powerfully and beautifully described a pestilence, from which we are tempted to give the following short extracts, to show that, 1500 years before the council of Trent, in addition to a knowledge of endemics, and epidemics, contagion was allowed to have its share in the spreading of the disease.

“Nam quid Britannis cælum differre putamus,
Et quod in Ægypto est, qua mundi claudicat axis?
Quidve quod in Ponto est, differre et Gadibus, atque
Usque ad nigra virum, percocto sæcla calore?”

LUCRET. *de Rerum Natura*, Lib. vi. l. 1104.

* See our Reviews of *Dr. Barker's Report*, &c. in our 58th Number, and of *Dr. Stokes on Contagion*, in No. 59.

“Est Elephas morbus, qui propter flumina Nili
Gignitur Ægypto in media, neque præterea unquam.”

LUCRET. *de Rerum Natura*, Lib. vi. l. 1112.

“Et, cum spiranteis mistas hinc ducimus auras
Illa quoque in corpus pariter sorbere necesse est.”

l. 1127.

“Quippe et enim nullo cessabant tempore apisci
Ex aliis alios avidi contagia morbi.”

l. 1234.

“Qui fuerant autem præsto, contagibus ibant
Atque labore.”

l. 1241.

The belief that the plague is contagious, is not in the least invalidated by the fact that many persons have escaped after an exposure that might have been expected to produce infection, since the same may be urged against small pox, which Dr. Maclean admits to be a contagious disease; and he must also admit, that, whilst many who are exposed to that contagion contract the disease, there are likewise many who do not; a remark which applies also to inoculation in both. The question has been repeatedly asked, Whether our long immunity from plague were sufficient ground for abrogating the quarantine laws? But it will be seen, that, although the island of Malta is, from many causes, much more exposed to this infection than Britain, yet it was 138 years free from plague, and we have exceeded that period only sixteen years. The danger of altogether abolishing quarantine regulations is great and obvious; but, at the same time, we have no doubt they might be amended, and rendered less inconvenient to commerce.

We shall conclude by giving a table, which will place in a forcible light the contrariety of opinion amongst the persons examined by the Committee.

	If con- tagious.	If conta- gious by Goods.	If Qua- rantine is of use.	If an eruptive Disease.	If the Plague of 1665 were true Levant Plague.	If from personal Observa- tion.
Sir James M'Grigor	Yes	Yes	Yes	Yes	Yes	Yes
Sir B. Faulkner	Yes	Yes	Yes	Yes	Yes	Yes
Dr. Pym	Yes	Yes	Yes	Yes	Yes	No
Dr. Granville	Yes	Yes	Yes	Always	Yes	Yes
Dr. Powell	Yes	Yes	Yes			No
Dr. Ash	Yes	Yes	Yes			No
Dr. Latham	Yes	Yes	Yes			No
Dr. Frank	Yes	Yes	Yes			Yes
Dr. Frampton	Yes		Yes	Yes		No
Dr. Harrison	Yes	Yes			Yes	No
Mr. Hayes	Yes	Yes				Yes
Dr. Tainsh	Yes			Yes		Yes
Sir R. Wilson	Yes					Yes
Dr. Johnson	Yes	Yes	Yes	Always	Doubtful	No
Dr. Forster	Yes	Yes	Yes	Yes	No	No
Dr. Gladstone	Yes	Yes	Yes	Generally	No	Yes
Dr. Nevinson	Yes	Yes	Yes	No	Yes	No
Dr. M'Leod	Yes	Yes	Doubtful	Yes	Yes	No
Dr. Curry	Yes	Improbable	Yes		Yes	No
Mr. Green	Yes	No	Yes	Yes	Yes	Yes
Dr. Maclean	No	No	No		No	Yes
Dr. Mitchell	No	No	No	No	No	No

Further observations on the Internal Use of the Hydrocyanic (Prussic) Acid in Pulmonary Complaints, Chronic Catarrhs, Spasmodic Coughs, Asthma, Hooping-Cough, and some other Diseases. With full directions for the Preparation and Administration of that Medicine. By A. B. GRANVILLE, M. D. F. R. S. F. L. S. M. R. I. Physician in ordinary to His Royal Highness the Duke of Clarence; Licentiate of the Royal College of Physicians of London; and Physician-Accoucheur to the Westminster General Dispensary. London, 1819. Octavo, pp. 82.

[From the Medico-Chirurgical Journal, for October, 1819.]

DR. GRANVILLE, after detailing the modes of preparing, and the physical properties of, the prussic acid, prefaces his account of its effects upon the human frame by remarking, that though poisonous in itself, like many of our best remedies, it may be used with perfect safety and advantage when properly administered; and, that no case has yet been recorded in which it has proved either fatal or injurious.

Another objection which he admits to be better founded, viz. that it has failed in producing any good effect in some of the complaints in which it has been exhibited, the author thinks only goes to prove a want of sufficient knowledge in the person employing it; but, we would ask, may it not also, peradventure, prove the incompetency of the medicine employed? The following are the effects of this remedy as stated by Dr. Granville:

“The prussic acid is evidently sedative, more so even than opium; but its specific mode of action is somewhat different, both as to its progress and effect, from that of the latter substance. The prussic acid, when administered to a patient exhausted by disease, and by the means employed to cure it, appears to exert an immediate influence upon the nervous system; it gradually diminishes all irritability, checks a too rapid circulation, and calms many of the symptoms of fever. If a dry cough be present, it promotes expectoration in the first instance, and subsequently stops the cough itself. The spirits, before exalted, soon feel the quieting impression of the acid; they become subdued; the speech, countenance, even the expression of the eyes assume a character of unusual meekness; there is a relief from pain and actual suffering; the patient feels it, and is grateful: sleep comes on undisturbed, respiration is soft, and the pulse more quiet than at other periods of the complaint, having lost the thumping beat of irritation. In some few cases these sedative effects are so much more considerable, that the patient expresses that he feels himself as if only *half alive*. On these occasions there is an *apparent* entire prostration of strength, great lowness of spirits, and unwillingness to move, speak, or take food; life seems suspended, yet the head and mind remain clear and intelligent; there is a total absence of pain; neither does the patient complain of any symptom of local or general irritation; the heat of the skin is natural, and the pulse, in the midst of this dead suspense, continues its course steadily and quietly. This state of things lasts from twelve to twenty-four hours, when it ceases; and every organ is gradually restored to its former elasticity.

“But it should be borne in mind that such instances of great depression, produced by the acid, are extremely rare, and in-

deed seldom occur where tonics, or a proper quantity of nourishment can be given to the patient, at the time he is taking the acid. There is scarcely a remedy which does not, more or less, present some anomalies in its effects, dependant on the particular idiosyncrasy of the patient. In some few cases the prussic acid disagrees with the stomach, and then it neither can, nor ought to be, persisted in: it also occasions giddiness in some individuals; in which case it is necessary either to discontinue it, or diminish the dose, and associate it with slight stimulants. Opium, henbane, &c. have often, in this respect, exhibited the same occasional deviations from their usual mode of action.

“ But the prussic acid has never yet been found to produce the head-ache and heaviness occasioned by laudanum; the fluttering and palpitations brought on by hemlock; nor the parched mouth and irritability of the throat, arising from the action of belladonna. The prussic acid, moreover, acts gently on the bowels, in the first instance, and when after some days they seem to fall into a torpid state, the mildest medicines, and those in smaller quantities than usual, suffice to produce the desired effect, when combined with the acid.”

The author next proceeds to enumerate the diseases in which it has proved beneficial. Its effects “ in almost every kind of cough, particularly of a spasmodic nature, are highly satisfactory.” “ In hectic fever it affords ease; lowers the pulse; diminishes the number of paroxysms; works a favourable change in the action of the lungs and their circulation, while the morbid heat of the skin, and the circular flushes of the cheeks, gradually disappear. The night sweats are also soon suspended.” p. 21. “ In the treatment of confirmed consumption, the prussic acid, even at the approach of death, is the most advantageous palliative that can be employed. Greatly superior, indeed, to any hitherto adopted.” p. 22. Asthmatic complaints are also stated to have been greatly relieved by it; and from analogy, the author is sanguine enough to expect much benefit from it in spasms of the stomach and diaphragm, and even in “locked jaw, tic douloureux, and, perhaps, hydrophobia!” p. 23. “Credat Judæus appella.” The following

instance, however, we can fully credit: "but I have had no experience of its utility in such affections."

Dr. G. has successfully employed this medicine in the dry convulsive spasmodic cough, which may be called sympathetic, as depending entirely on a morbid state of the liver, stomach, spleen, &c. With regard to hooping cough, "it may be stated, without presumption, that no case need be suffered to proceed longer than eight or ten days if the prussic acid be timely and cautiously administered; and, it is singular, that children bear the action of this sedative medicine, in small doses, better than adults." p. 24. In inflammatory affections of the lungs, which have a tendency to recur on the slightest cause, and to terminate in hectic and purulent expectoration, and after depletion cannot be carried any further, Dr. G. thinks the prussic acid will produce the most advantageous effects; and also in those pains which attend and follow abortions, and in hæmoptysis. p. 25, 6, 7. Some cases follow, illustrative of its good effects in consumptive and other cases above mentioned, by the author, Dr. Majendie, Mangini, Scudamore, and Mr. Thompson, of Sloane street; and, according to their observations, no bad effect had ever resulted from this medicine, though great care is admitted to be requisite in its exhibition.

Towards the conclusion of the volume is a valuable communication from the latter gentleman, with a detail of four cases, in which he exhibited this medicine. He observes, "I have prescribed the acid in a great number of instances, with variable success; but the benefit it has produced is amply sufficient to authorise me to add my testimony in favour of its value as an important addition to the host of direct sedatives." p. 67.

The 8th section is occupied with the mode of prescribing the prussic, or hydrocyanic acid. It decomposes most of the salts used in medicine, particularly those of antimony and soda. Its affinity for the alkalies and earths is inconsiderable; that for the former is so weak, that even carbonic acid displaces it. Hence it may be given with carbonate of potash, forming one of the most successful modes of prescribing it in spasmodic and hooping coughs. It cannot be administered with the sulphurets. It may be given in vegetable infusions,

with the addition of syrups if necessary. With tonics it may be associated with real advantage, as with the filtered infusions of bark, columbo, cascarilla, or even sarsaparilla, or in incipient pulmonary complaints in a strong decoction of lichen islandicus. It is indispensably necessary to use no other than distilled water in all prescriptions with the prussic acid; otherwise decomposition will take place. p. 60, 63. Some formulæ follow, in which eight or ten minims of the acid are exhibited in six or eight ounces of the vehicle; a table-spoon full every two or three hours. p. 63. When the prussic acid produces nausea, vomiting, or dizziness, which it will do in some individuals on the very first or second day, it is advisable to abandon it immediately, for there is no chance of its ever agreeing with the patient; but such effects have not occurred above five or six times in one hundred cases in which it has been exhibited. p. 39. During the first days it proves gently aperient; and when it has this effect, the mildest purgatives will suffice to produce the necessary evacuation. Mr. Thompson states his general practice in catarrhal affections and chronic coughs to have been, after purging, to give two minims of the acid in a spoonful of distilled water or almond emulsion, every two or three hours during the day, increasing the quantity two or three minims on the whole portion taken in the twenty-four hours, every day, until the cough was subdued. p. 70. The greatest amount to which it has been carried by him was twenty-four minims in the day to an adult, and six to an infant. For infants between four months and a year old, he prescribed two minims in ℥ix of distilled water, with ℥j of the syrup of Tolu, coch. min. jī. 3tiis. horis sumenda. As to its *modus operandi*, he observes; "the prussic acid, when taken into the stomach, produces its action on the circulating system, evidently through the medium of the nerves, the energy of which it considerably lessens and even altogether destroys when the dose is sufficiently strong. In no case have I remarked that any excitement precedes its sedative effect, a circumstance which distinguishes it from every other substance belonging to the class of narcotics." p. 67.

There are some typographical mistakes which ought to have been noticed in a table of errata, as in the formula, page 76,

and at page 81, *obnoxious* is used instead of *noxious*. In presenting a short but faithful analysis of this little volume, we will not say, "*cras credemus, hodie nihil*," but we shall feel much contented, and deem the prussic acid no contemptible acquisition, if experience shall prove it to deserve even a moderate competency of the virtues and efficacy here ascribed to it. We recommend this little work to the perusal of our brethren.

Reports on the Epidemic Cholera which has raged throughout Hindostan and the Peninsula of India, since August, 1817. Published under the Authority of Government. One Vol. 4to. 228 Pages. Bombay, 1819.

[From the Medico-Chirurgical Journal, for April, 1820.]

— seu dira per omnes
Manarent populos sævi contagia morbi.

THIS important series of documents, drawn up by the Medical Board of Bombay, has been presented to us, through the medium of Dr. Scott, by the desire of the head of that board, lately returned to Europe.* The work is circulating widely in India, but cannot, of course, be known here, except through such vehicles as the present. We deem it a duty, therefore, to the profession at large, to make them more intimately acquainted than they have hitherto been, with one of the most awful and fatal epidemics that ever ravaged our widely extended Indian empire. The event itself is extremely interesting to the profession in general, in a pathological and therapeutical point of view, independently of those numerous ties and associations by which we are linked to the fate of our Asiatic possessions. On all these accounts we shall be pardoned for the length to which our analysis may extend, especially as we shall strain every nerve to make it as concentrated as literary labour and typographical closeness can render it.

There are some curious particulars attending the history of

* Dr. Steuart, since deceased.

this epidemic, which are worthy of record. It first appeared in August, 1817, in Zilla Jessore, about 100 miles North East of Calcutta, but without any previous peculiarity of weather; being considered by the authorities on the spot, as of a local nature, and attributable to the intemperate use of rank fish and bad rice; but it rapidly spread through the adjoining villages, running from district to district, until it had brought the whole province of Bengal under its influence. It next extended to Behar; and, having visited the principal cities west and east of the Ganges, reached the upper provinces. Through the large cities here it made a regular progress; but it was otherwise in the more thinly peopled portions of country. "The disease would sometimes take a complete circle round a village, and leaving it untouched, pass on as if it were wholly to depart from the district. Then, after a lapse of weeks, or even months, it would suddenly return; and scarcely re-appearing in the parts which had already undergone its ravages, would nearly depopulate the spot that had so lately congratulated itself on its escape. Sometimes, after running a long course on one side of the Ganges, it would, as if arrested by some unknown agent, at once stop; and taking a rapid sweep across the river, lay all waste on the opposite bank." *Report of the Calcutta Medical Board.*

In Calcutta it shewed itself in the first week of September, and each succeeding week added strength to the malady, and more extended influence to its operation. From January till the end of May it was at its acmé; during which period, the mortality in the city was seldom under two hundred a week!

The centre division of the army, under the commander in chief, exhibited an awful specimen of the fatality of the disease. It consisted of less than ten thousand fighting men, and the deaths, within twelve days, amounted, at the very lowest estimate, to three thousand; according to others, to five, and even eight thousand!

On the 6th of August, 1818, it reached Bombay, taking about a year to cross the base of the Great Indian Delta. It appeared to Drs. Steuart and Phillips, the enlightened members of the medical board at Bombay, that the disease was capable of being "transported from place to place as in cases of

ordinary contagion or infection, and also to possess the power of propagating itself by the same means that acknowledged contagions do." *Preface*, xii.

The partial and irregular manner in which the disease spread and operated in the neighbourhood of Bombay, as the cold season advanced, could not be accounted for by the medical board, "unless by supposing that a diminution of temperature, together with exposure, may have called into action some latent remains of an active poison." The board next proceeds to a description of the disease, as drawn up by the medical board of Bengal, which we shall here introduce verbatim.

"Having thus given a rapid and imperfect sketch of the history of the epidemic, the board should now proceed to detail the symptoms which attended its attack. This part of their task they will not find it difficult to accomplish. The leading appearances of this most fatal malady were but too well marked on their approach and subsequent progress; and, amongst the myriads who were attacked, exhibited perhaps less variety and fewer discrepancies than characterize the operation of almost any other disease to which the human body is subject. The healthy and unhealthy; the strong and feeble; Europeans and natives; the Mussulman and Hindoo; the old and young of both sexes, and of every temperament and condition, were alike within its influence.

"The attack was generally ushered in by a sense of weakness, trembling, giddiness, nausea, violent retching, vomiting and purging, of a watery, starchy, whey-coloured, or greenish fluid. These symptoms were accompanied, or quickly followed by severe cramps, generally beginning in the fingers and toes, and thence extending to the wrists and fore-arms, calves of the legs, thighs, abdomen, and lower part of the thorax. These were soon succeeded by pain, constriction, and oppression of stomach and pericardium; great sense of internal heat; inordinate thirst, and incessant calls for cold water; which was no sooner swallowed than rejected, together with a quantity of phlegm, or whitish fluid, like seethings of oatmeal. The action of the heart and arteries now nearly ceased; the pulse either became altogether imperceptible at the wrists and tem-

ples, or so weak as to give to the finger only an indistinct feeling of fluttering. The respiration was laborious and hurried, sometimes with long and frequently broken inspirations. The skin grew cold, clammy, covered with large drops of sweat; dank and disagreeable to the feel, and discoloured, of a bluish, purple, or livid hue. There was great and sudden prostration of strength, anguish, and agitation. The countenance became collapsed; the eyes suffused, fixed and glassy; or heavy, and dull; sunk in their sockets, and surrounded by dark circles; the cheeks and lips livid and bloodless; and the whole surface of the body nearly devoid of feeling. In feeble habits, where the attack was exceedingly violent, and unresisted by medicine, the scene was soon closed. The circulation and animal heat never returned; the vomiting and purging continued, with thirst and restlessness; the patient became delirious or insensible, with his eyes fixed in a vacant stare, and sunk down in the bed; the spasms increased, generally within four or five hours.

“The disease, sometimes at once, and as if it were momentarily, seized persons in perfect health; at other times, those who had been debilitated by previous bodily ailment; and individuals in the latter predicament, generally sunk under the attack. Sometimes, the stomach and bowels were disordered for some days before the attack, which would then, in a moment, come on in full force, and speedily reduce the patients to extremities.

“Such was the general appearance of the disease where it cut off the patient in its earlier stages. The primary symptoms, however, in many cases, admitted of considerable variety. Sometimes the sickness and looseness were preceded by spasms; sometimes the patient sunk at once, after passing off a small quantity of colourless fluid, by vomiting and stool. The matter vomited in the early stages was, in most cases, colourless or milky; sometimes it was green. In like manner, the dejections were usually watery and muddy; sometimes red and bloody; and in a few cases, they consisted of a greenish pulp, like half digested vegetables. In no instance was feculent matter passed in the commencement of the disease. The cramps usually began in the extremities, and thence gradually

crept to the trunk; sometimes they were simultaneous in both; and sometimes the order of succession was reversed; the abdomen being first affected, and then the hands and feet. These spasms hardly amounted to general convulsion. They seemed rather affections of individual muscles, and of particular sets of fibres of those muscles, causing thrilling and quivering in the affected parts, like the flesh of crimped salmon, and firmly stiffening and contorting the toes and fingers. The patient always complained of pain across the belly, which was generally painful to the touch, and sometimes hard and drawn back towards the spine. The burning sensation in the stomach and bowels was always present, and at times extended along the cardia and œsophagus to the throat. The powers of voluntary motion were, in every instance, impaired, and the mind obscured. The patient staggered like a drunken man, or fell down like a helpless child. Head-ach over one or both eyes sometimes, but rarely occurred. The pulse, when to be felt, was generally regular, and extremely feeble, sometimes soft; not very quick; usually ranging from 80 to 100. In a few instances, it rose to 140 or 150, shortly before death. Then it was indistinct, small, feeble, and irregular. Sometimes very rapid, then slow for one or two beats. The mouth was hot and dry; the tongue parched, and deeply furred, white, yellow, red, or brown. The urine at first generally limpid, and freely passed; sometimes scanty, with such difficulty as almost to amount to strangury; and sometimes hardly secreted in any

warm, whilst the limbs kept deadly cold. The pulse would return; grow moderate and full; the vomiting and cramps disappear; the nausea diminish, and the stools become green, pitchy, and even feculent; and with all these favourable appearances, the patient would suddenly relapse; chills, hiccup, want of sleep, and anxiety, would arise; the vomiting, oppression, and insensibility, return; and in a few hours terminate in death.

“ When the disorder ran its full course, the following appearances presented themselves. What may be termed the cold stage, or the state of collapse, usually lasted from twenty-four to forty-eight hours, and was seldom of more than three complete days’ duration. Throughout the first twenty-four hours, nearly all the symptoms of deadly oppression, the cold skin, feeble pulse, vomiting and purging, cramps, thirst and anguish continued undiminished. When the system shewed symptoms of revival, the vital powers began to rally; the circulation and heat to be restored; and the spasms and sickness to be considerably diminished. The warmth gradually returned; the pulse rose in strength and fullness, and then became sharp and sometimes hard. The tongue grew more deeply furred; the thirst continued, with less nausea. The stools were no longer like water; they became first brown and watery; then dark, black, and pitchy; and the bowels, during many days, continued to discharge immense loads of vitiated bile, until, with returning health, the secretions of the liver and other viscera gradually put on a natural appearance. The fever, which invariably attended this second stage of the disease, may be considered to have been rather the result of nature’s effort to recover herself from the rude shock which she had sustained, than as forming any integrant and necessary part of the disorder itself. It partook much of the nature of the common bilious attacks prevalent in these latitudes. There was the hot dry skin; foul, deeply furred, dry tongue; parched mouth; sick stomach; depraved secretions, and quick variable pulse; sometimes with stupor, delirium, and other marked affections of the brain. When the disorder proved fatal after reaching this stage, the tongue, from being cream-coloured, grew brown, and sometimes dark, hard, and more deeply

furred; the teeth and lips were covered with sordes; the state of the skin varied; chills, alternating with flushes of heat; the pulse became weak and tremulous; catching of the breath; great restlessness, and deep moaning succeeded; and the patient soon sunk, insensible, under the debilitating effects of frequent dark, pitchy, alvine discharges.

“Of those who died, it was believed, perhaps rather fancifully, that the bodies sooner underwent putrefaction, than those of persons dying under the ordinary circumstances of mortality. The bodies of those who had sunk in the earlier stages of the malady, exhibited hardly any unhealthy appearance. Even in them, however, it was observed, that the intestines were paler, and more distended with air, than usual; and that the abdomen, upon being laid open, emitted a peculiar offensive odour, wholly different from the usual smell of dead subjects. In the bodies of those who had lived some time after the commencement of the attack, the stomach was generally of natural appearance, externally. The colour of the intestines varied from deep rose to a dark hue, according as the increased vascular action had been arterial or venous. The stomach, on being cut into, was found filled, sometimes with a transparent, a green, or dark flaky fluid. On removing this, its internal coats, in some cases, were perfectly healthy; in others, and more generally, they were crossed by streaks of a deep red, interspersed with spots of inflammation, made up of tissues of enlarged vessels. This appearance was frequently continued to the duodenum. In a very few cases, the whole internal surface of the stomach was covered with coagulable lymph; on removing which, a bloody gelatine was found laid on the interior coat, in ridges or elevated streaks. The large intestine was sometimes filled with muddy fluid, sometimes livid, with dark bile, like tar; just as the individual had died in the earlier or later periods of the attack. In most cases, the liver was enlarged, and gorged with blood. In a few, it was large, soft, light-coloured, with greyish spots, and not very turgid. In others again, it was collapsed and flaccid. The gall-bladder was, without exception, full of dark green or black bile. The spleen and thoracic viscera were, in general, healthy. The great venous vessels were usually gorged;

and in one case, the left ventricle of the heart was extremely turgid. The brain was generally of natural appearance. In one or two instances, lymph was effused between its membranes, near the coronal suture, so as to cause extensive adhesions; in other cases, the sinuses, and the veins leading to them, were stuffed with very dark blood." xv.—xxi.

The following extracts will show that the disease was known to Sydenham, and accurately described by that observant physician. He no where mentions bile as forming any part of the discharges from the stomach or bowels; and hence, it may be fairly inferred, that such discharges were not present.*

"Qui ab ingluvie ac crapula nullo temporis discrimine passim excitatur affectus, ratione symptomatum non absimilis, nec eandem curationis methodum respuens, tamen alterius est subsellii. Malum ipsum facile cognoscitur, adsunt enim vomitus enormes, ac pravorum humorum cum maxima difficultate et angustia per alvum dejectio; cardialgia, sitis. Pulsus celer ac frequens, cum æstu et anxietate, non raro etiam parvus et inæqualis, insuper et nausea molestissima, sudor interdum diaphoreticus, crurum et brachiorum contractura, animi deliquium, partium extremarum frigiditas, cum aliis notæ symptomatibus, quæ adstantes magnopere perterrefaciunt, atque etiam angusto viginti quatuor horarum spatio ægrum interimant."

And again, in his letter to Dr. Brady, describing the epidemics of 1674, 5, and 6, he says,

"Exeunte æstate Cholera Morbus epidemice jam sæviebat,

* We have diligently searched the writings of Sydenham, and we assert, that in no one instance, when treating of cholera morbus, whether epidemic or sporadic, has he mentioned a discharge of *bile* as forming any part, much less as being the *cause* of cholera. And as Sydenham is allowed to be one of the most accurate observers of nature, we see on what foundation Dr. Saunders and others have built their *bilious* theory of the disease. The fact is, as we have long ago stated, that the discharge of bile in cholera, is a secondary or ternary link in the chain of cause and effect—and always a sanative effort of the system, as well as a favourable symptom of the disease.

We observe too, that Aretæus describes the discharge of *bile* as only an ulterior effect. "In primis," says he, "quæ evomuntur, aquæ similia sunt; quæ anus effundit, stercorea, liquida, tetricæ odoris sentiuntur. Siquidem longa cruditas id malum excitavit, quo si per clysterem eluantur, primo pituitosa, mox biliosa feruntur."—*De Cholera*, Chap. 5.

et insueto tempestatis calore evector, atrociora convulsionum symptomata, eaque diuturniora secum trahebat, quam mihi prius unquam videre contigerat. Neque enim solum abdomen, uti alias in hoc malo, sed universi jam corporis muscoli, brachiorum crurumque præ reliquis, spasmis tentabantur dirissimis, ita ut æger e lecto subinde exiliret, si forte extenso quaquaversum corpore eorum vim posset eludere." xxiii.

The first of the foregoing extracts describes the disease with great accuracy, as it very generally affected the natives; the second is well exemplified in Dr. Burrell's Report, as it attacked the Europeans of the 65th regiment, at Seroor. The disease is also accurately described by Girdlestone, and by Mr. Curtis of Madras, in 1782, when it raged in the southern provinces of the peninsula. Dr. Taylor also furnished the Medical Board with the account of a disease from a Sanscrit medical work, the MADHOW NIDAN, which clearly proves that the complaint has been long known to the natives.

"It is obviously unnecessary to prosecute this inquiry further; and we shall only add, that Dr. James Johnson is the latest author, so far as we know, who has treated this subject, and who has also the merit of having been the first who has generally pointed out the best method of cure, from a few cases he met with on the eastern coast of Ceylon, where the disease seems to be more prevalent than in any other part of India." xxviii.

The exciting and approximate causes of this interesting epidemic are, like those of most others, concealed in utter darkness—"atraz caligine mersæ;" great discrepancy of opinion obtains in India respecting its contagious or non-contagious influence, arising naturally out of the difficulty of the subject.

"Several irresistible facts already noticed, or related in the following reports, and its marked anomaly from all hitherto known simple epidemics, would seem to favour the doctrine of contagion, while the contrary supposition is only supported by a species of negative evidence." xxix.

The board, however, very properly observe, that this is a question of such importance, that it ought not to be too hastily entertained as proved, nor rejected as unfounded; but

prosecuted with that diligent inquiry and cautious induction, which, on every subject of science, are so necessary to the attainment of truth.

In respect to the predisposing [or rather the *exciting*] causes, practitioners are unanimous.

“Rapid atmospherical vicissitudes, in regard either to temperature or moisture; exposure of the body to currents of cold air, particularly the chill of the evening, after being heated by violent exercise of any kind, inducing debility or exhaustion; low marshy situations; flatulent or indigestible food, especially crude and watery vegetables, which compose a large proportion of the diet of the natives; and particularly that gradual undermining of constitution which arises in a condensed, dirty, and ill-fed mass of population, are all unquestionably powerful predisposing causes.”

Sad experience, however, has shown that the absence of all these afforded no security against the attack. Whether the invisible cause, whatever that may be, acts more immediately on the vascular or nervous system, the board cannot take upon them to determine; but from the various modes of attack which gave rise to the division of the disease into two species and varieties, they are led to the supposition that sometimes the one system, sometimes the other, bears the onus of the first onset of the malady.

“The most general attack seems to consist in a spasmodic affection of the stomach, duodenum, and more especially the biliary ducts; the total absence of bile in the matter voided upwards and downwards being, perhaps, the most uniform characteristic of the disease, which, extending through the whole intestinal canal, discharges its contents. It is more than probable, however, that these are merely the first perceptible symptoms; for it would appear that a great change has already taken place in the circulating system, and that the action of the heart itself has been greatly diminished before they occur. This seems evident from the numerous cases in which neither vomiting nor purging is present, and in which the first appearance of the disease is the almost total suspension of the vital functions, immediately followed by severe spasmodic

affections of the muscles and coldness of the extremities.”
xxviii.

Here the board have copied Dr. Armstrong's description of the attack of *congestive* typhus, remarking that,

“Those who are most intimate with the disease in question, will be struck with the great similarity between this and typhus, at their first appearance.”

Dissections, they state, abundantly prove that venous congestion constitutes the principal change that takes place during life.

The following passage, though long, cannot be abridged, without greatly lessening its value.

“On the subject of the cure of the disease, we need say but little. The practice so judiciously and speedily adopted by Dr. Burrell in the 65th regiment, clearly proves, that at the commencement of the disease in Europeans, blood-letting is the sheet anchor of successful practice; and perhaps also with natives, provided it be had recourse to sufficiently early in the disease; and as long as the vital powers remain, so as to be able to produce a full stream, it ought perhaps never to be neglected; it having been sufficiently proved, that the great debility so much complained of is merely apparent. Calomel, as a remedy, certainly comes next in order, and when employed in proper doses, with the assistance of opium, and more particularly in the early stage of the disease, seems to be equally effectual among natives, as venesection among Europeans, in arresting its progress. In all the cases formerly alluded to, when we met the disease on its first attack, a single scruple dose of calomel, with sixty minims of laudanum, and an ounce of castor oil seven or eight hours afterwards, was sufficient to complete the cure. The practice of this place, as sufficiently appears by Dr. Taylor's report, bears ample testimony to the control which calomel possesses over the disease, in as much as it has often preserved life, when blood-letting could not be put in practice.

“All other remedies must, in our opinion, be considered as mere auxiliaries, no doubt extremely useful as such, and ought never to be neglected; but particularly the warm bath and stimulating frictions. Even where the disease appears to

have given way to bleeding, we think it highly necessary constantly to administer calomel. The powerful effect of this remedy in allaying irritability of the stomach and intestines, when given in large doses, is generally acknowledged by practitioners, in the severer attacks of dysentery: as a great and permanent stimulus to the vascular system, it will be readily acknowledged by every one who has suffered for any length of time under its effect in ptyalism, where the bounding pulsations of the arteries of the temples and neck produce very disagreeable sensations, and even preclude sleep. Its powers over inflammation of the abdominal viscera, the liver in particular, and indeed, in membranous and glandular inflammation generally, are now universally acknowledged.*

* "We read with some surprise the declared opinion of Dr. Armstrong, in his *Treatise on Puerperal Fever*, that the good effects of calomel were solely owing to its purgative quality; while, at the same time, he acknowledges that the disease was both more speedily and perfectly overcome in those cases where ptyalism was produced. He has, however, made ample amends in the treatise now before us, where he acknowledges that its value is to be attributed to its specific qualities as a mercurial. It is by the acknowledged errors of such men that medical practice is stored with its most valuable facts. Puerperal fever is not a very common occurrence in this country [India], although it occasionally takes place; and it is but an act of justice due to Dr. Helenus Scott, formerly of this place, now of Russel Square, to state, that for more than thirty years back, he was in the constant practice of treating this fever with calomel so as to affect the system; and that, to the best of our recollection he never lost a patient. We are led to make this observation from another motive, as we have reason to believe, that this salutary practice is but little known in this country, and it may serve as a hint which may save some valuable lives."

R. STEUART,
B. PHILLIPS.

The following Communication from MR. JOHN SCATCHARD, of East Keswick, near Leeds, will come in opportunely here.

"In the treatment of puerperal fever, by general professional consent, large doses of the submuriate of mercury are allowed to be of very great utility, especially when exhibited opportunely, and in concert with other well known auxiliary means. Dr. Armstrong in his valuable work on the above disease has very successfully employed this remedy, and thus recommends its more extensive adoption. It appears, however, from what we may collect from his work, p. 65, 1st. edit. and p. 10, 88, 94, 2d edit., that neither the author, nor any of his intelligent correspondents, had recourse to this medicine, in such quantity at least, previously to January, 1813.

"Now, if your readers will take the trouble to refer to the 145th and 157th numbers of the *Medical and Physical Journal*, particularly the former, it will be seen, that Dr. Bradley of Huddersfield, administered large doses of calomel in puerperal fever, with the most unequivocal success, at least two years before Dr. Armstrong adopted the same remedy. Moreover, a reference to these cases will demonstrate,

“In a disease, therefore, in which we have every reason to believe that venous congestion has taken place to a great extent, and where we conclude that the liver, from its peculiar circulation and structure, is more immediately liable to become seriously and permanently injured, it should not be omitted. We have before mentioned, that Dr. James Johnson seems to have been the first who pointed out the best method of cure. Since most of the foregoing remarks were written, we have seen the second edition of that gentleman’s valuable work, in which we find a strong corroborative testimony to the utility of blood-letting in this disease, or one somewhat similar to it, on the coast of Brazil, by Mr. Sheppard of Witney, without the assistance of any other remedy. The public are greatly indebted to Mr. Corbyn, of the Bengal establishment, for his clear and comprehensive letter on this subject, at a time when the disease was producing the most dreadful ravages: the early communication of his practice has been the means of saving thousands of lives in situations where Dr. Johnson’s work might not be known.” xlii.

About forty official reports, from various medical officers, compose the great body of the work before us, and form the materials from which Drs. Steuart and Phillips have drawn up the foregoing luminous and interesting digest. It is not ne-

that in Dr. Bradley’s hands, a combination of calomel, with opium, was beneficially employed in one instance; and in the second edition of his work, Dr. Armstrong has fully canvassed this remedy. Whether the striking coincidence herein evinced were fortuitous or otherwise, it is impossible to pronounce with certainty.

“The priority of adoption and publication of the above mode of exhibiting calomel, for the cure of puerperal fever, I believe, unquestionably belongs to Dr. Bradley.

“The two papers above referred to, contain the essence of almost every thing valuable that has been written on the subject of puerperal fever, since Dr. Gordon’s inestimable publication on the same disease. In those papers, blood-letting and copious purging are inculcated; and more definite criteria, respecting the indications and proper period for having recourse to the former remedy, are there pointed out, than are elsewhere to be met with. And what, I would ask, has the science of medicine, as yet, added to these means of cure, strictly speaking?

“Should Dr. Armstrong peruse these remarks, he will pardon the freedom with which they are made, and be assured that no one can have a more profound respect for his great talents than I entertain; yet, since every man is entitled to the benefit or credit of his own discoveries, I have deemed it but right unreservedly to assert the prior claim of its legitimate author to the above improved practice.”

JOHN SCATCHARD.

cessary to go into these reports individually. There never perhaps existed so unanimous a consent respecting the treatment of such a wide-spreading epidemic, as these documents disclose. The pre-eminent powers of blood-letting and mercury in diseases of the eastern world, are now so firmly established on the basis of facts, that it would be ingloriously to tread over a prostrate enemy, to even notice the dreams of the Brunonians, and the scepticism of the anti-mercurialists.

Medical Notes on Climate, Diseases, Hospitals, and Medical Schools, in France, Italy, and Switzerland; comprising an Inquiry into the Effects of a Residence in the South of Europe, in Cases of Pulmonary Consumption, and illustrating the present State of Medicine in those Countries. By JAMES CLARK, M. D. London, 1820. pp. 250. Octavo.

[From the Medico-Chirurgical Journal, for April, 1820.]

WE were lately so completely *taken in* by a goodly volume bearing a title something similar to the one now before us, that it was not without some hesitation that we ventured upon the purchase and perusal of Dr. Clark's. We were indeed, somewhat re-assured by the modesty of the title page, and the unpretending humbleness of the preface; but we have waxed too wise and sober on our critical Pharos to risk much of our judgment on such appearances—*fronti nulla fides*—and it was only after having clandestinely run over, in our bookseller's, some of the pages in the *interior*, that we ventured to order its translation to our own shelves. In this instance, we are happy to say that we have not been disappointed. Dr. Clark gives us more than he promises but certainly much less than we could wish. Indeed, on many occasions his remarks are so brief and few, that even the title of the volume (Notes) and the apologetical circumstances mentioned in the preface, were insufficient to satisfy our craving for further information, which was only irritated by the little that was afforded us. Upon the whole, however, we have been much pleased and

instructed by this little work of Dr. Clark, and we can very conscientiously recommend it to our readers, as containing much useful and interesting matter.

It appears by the preface, which is in the form of a letter, dated Rome, Nov. 1819, and addressed to Dr. Forbes of Penzance, that the Notes now offered to the public were taken under very varying circumstances, over which the author had frequently no control, of time and convenience, during casual visits to the places noticed; and that they are now presented to the public chiefly at the instance of the author's friends, from a conviction that they contain matter both valuable and interesting, and from apprehension lest events may prevent the accomplishment of the hope he entertains of being hereafter enabled to fill up the outline he has sketched.

Dr. Clark's book is divided into two parts: the first comprehending his observations on the climate and endemic diseases of some parts of the south of France, Italy and Switzerland; the second his account of the state of medicine in some of the hospitals and medical schools of the same countries. In collecting the materials for the first part of his work, the object of Dr. Clark has evidently been to endeavour to ascertain the effect of the climates of the south of France and Italy on those cases of consumption sent thither annually, in such numbers from England; to determine whether this practice is at all useful or injurious; in what cases and stages of the disease it is so; and which, of all the various places resorted to, is the most eligible. In the fulfilment of this plan, in addition to the examination of the topography and local habitudes of the different places, he has naturally been led to inquire into the nature and degree of prevalence of the diseases met with in them, especially of such as have any relation to consumption, or to those qualities of climate found to influence its development or progress. In this inquiry we are frequently presented with very interesting notices of diseases not particularly connected with the main subject of investigation; also with curious dietetic notices, and valuable practical remarks. We are, also, presented with numerous highly useful directions for the guidance of the invalids in the different places frequented by them, and many cautions and warnings, highly

important to this class of sufferers; and which, by the earnestness with which they are endeavoured to be impressed, very advantageously illustrate the feeling and kind-heartedness of the excellent author. In reference, particularly to this part of the book, we consider it as constituting a most excellent guide for the invalid who visits these countries, and earnestly recommend it to all such, and to professional men who may accompany them, as fraught with much that it imports them to know, and which they cannot otherwise acquire than by much and hazardous experiment.

Dr. Clark's experience has led him to form a very low estimate of the beneficial influence of the climate of the south of France and Italy in pulmonary consumption. Indeed, the impression left on our minds by an attentive perusal of his work, of the inutility, nay, of the impropriety of sending consumptive patients thither from this country, is very strong, much stronger than that which seems to have been left on the mind of the author himself; certainly infinitely stronger than our previous imperfect information on this matter had led us to entertain. The author sums up his opinion on this subject in the following words, in which we are, in the main, disposed to agree with him.

“To sum up in a few words the opinion I have formed from all the observations I have been enabled to make on the effects of climate in pulmonary consumption, it appears to me, then, that the change of our English climate for a residence in the milder ones of the south of Europe, is much more beneficial as a preventive of the disease, than, I fear, it will ever be found as a means of cure of it when formed. In the young and growing members of delicate, scrofulous and consumptive families, however, continued for some winters during that age when the body is attaining its full growth, and when catarrhal affections are attended with the greatest danger, it may have great influence in checking the tendency to hereditary disease. Even when tubercles already exist in the lungs in a state of irritation, a residence for some years in a mild temperature, together with the adoption of a proper regimen, may be the means of allaying the irritation, and consequently of preventing the suppuration of these tubercles. By a little fu-

ture attention in guarding against the known exciting causes of inflammation, these may long, and perhaps for life, remain in a state of quiescence. By such measures, and a strict adherence to the other means most proper for strengthening the constitution, and by acquiring habits calculated to inure the body to the cold and inequalities of its native climate, among which I consider the habitual use of the cold bath as pre-eminent, I have no doubt that many lives might be saved. When, however, suppuration has actually taken place in the substance of the tubercles, my opinion is, that little or no benefit is to be expected from a change of climate in the cure of the disease; and further, that by the great and numerous inconveniences and discomforts of so long a journey, the fatal termination of it is more frequently accelerated than protracted. That this is very frequently the case in the very advanced stages of the disease, such as I have frequently met with on the continent shortly after their arrival from England, I have no manner of doubt." p. 114.

The places whose fitness for winter residence for the consumptive are investigated by Dr. Clark, are Marseilles, Hieres, Nice, Villa Franca, Pisa and Rome; and the plan he has followed in his examination of these appears to us very judicious. It is thus briefly stated by himself, and he seems to have faithfully adhered to it.

"I shall give in the ensuing remarks, first, a short topographical account of each, confining myself, however, strictly to those circumstances which appear to me immediately connected with its locality in a medical point of view; secondly, observations on its climate; and, thirdly, remarks on the diseases in which it seems useful or injurious, founded on these observations, and on a knowledge of the ailments to which the inhabitants are most liable. To these will be occasionally added a few remarks on the present state of medical practice, as far as sufficient information could be gained on that point." p. 7.

Of all the places described, Marseilles seems to be, on every account, the most improper as a residence; and, indeed, the objections to it detailed by Dr. Clark are so great and obvious, that it would be truly most surprising that it should still continue to be resorted to by any of our countrymen,

were it not for the very remarkable ignorance of the profession in this country, of the nature of the climate and local habitudes of the whole south of France. This ignorance we do not say that Dr. Clark's little work will entirely remove; indeed, it is much too brief to have this effect; but it is certainly more calculated to do so than any other work we have yet met with.

Marseilles is very much exposed to the violent, cold, piercing N. W. winds, termed by the natives *mistral*; which, indeed, are felt throughout the whole northern shores of the Mediterranean with remarkable severity.

"The great evil of Marseilles, as a winter residence to the consumptive, or those liable to inflammatory affections of the chest, is the frequency and force of the dry cold northerly winds; to which, during that season, it is particularly subject, and to the full influence of which its northerly aspect exposes it in a peculiar manner. The effect of these winds, the most frequent and violent of which is the north-west, termed by the inhabitants the *mistral*, is, to produce a very great, and often very sudden alteration of temperature. This is severely felt by the inhabitants themselves, the surface being probably relaxed, and rendered particularly sensible, by the preceding heats, and on many occasions cannot be guarded against by the most cautious invalid. The influence of this wind in sinking the thermometer is by no means in proportion to its effects on the living body."

This unfavourable exposure, and cold variable climate, appears to be extremely productive of disease of the pulmonary organs. Consumption is more prevalent in Marseilles than in most of the towns of France; its progress is stated to be extremely rapid; and is considered by the medical men as invariably fatal.

Females from 14 to 18 years of age are stated to be its most frequent victims. Its ravages among the youth are stated in very energetic terms by Dr. Segand, in a memoir quoted by our author. "*Elle fait des ravages inouis en moissonnant la plus belle jeunesse!*" p. 14.

Another evil of this place, considered as a residence for invalids, is said to be the local configuration of its vicinity, which

is extremely confined and hemmed in by the neighbouring mountains, and by the thick high enclosures of the fields and gardens. p. 10.

Hieres, a place of much higher repute as a station for the consumptive, is admitted by our author as justly entitled to this comparative praise; but he is by no means disposed to fall in with the high opinion entertained by many of its actually great advantages in this disease. "Its exposure is better, and it is in some degree protected by its finely clothed hills from the northerly winds, which, if not warded off, are at least broken in their violence." p. 25. This is comparatively good report, but even this is weakened by a remark in the preceding page.

"It is not well sheltered from the north-east, and still less so from the north-west, which last we have noticed as blowing during the greater part of December, so as to render it unsafe for a consumptive patient to venture out of doors, except in some corner, where he is exposed to the sun's rays, and completely sheltered from it." p. 24.

Like most of the towns on the southern shores of France, *Hieres* is surrounded on the land side by hills, and as these lie to the north of it, they tend to shelter it much more than *Marseilles*. The country around it is described as being extremely beautiful and fertile, but it seems the state of the cultivation, and inconveniencies of travelling, as at *Marseilles*, prevent invalids enjoying the advantages that the natural configuration of the place seems to afford in some situations.

"It is true, that about the bases of the hills there are some spots sheltered from the blasts of the mistral, where the invalid might enjoy several hours in the open air almost every dry day, but the difficulty is to reach them at the time that they would be most useful. The chilly blast, sweeping round every exposed corner, forbids the valetudinarian venturing there, except in a close carriage, while, on the other hand, the roads leading to these places do not admit wheeled carriages." p. 21.

Consumption is said to be much less frequent than at *Marseilles*.

The next place described by Dr. Clark, is *Nice*, which is

also situated close on the shore, and is bounded on the whole of the land sides by an amphitheatre of lofty mountains. The topographical advantages of this place, considered as a residence for the consumptive, seem to be very considerable, and much superior to any of the stations in the south of France. From the arrangement of its mountains, above noticed,

“Nice derives the superior mildness of its climate. While, open to the genial influence of the south, its mountain barrier on all other sides defends it in a great measure from the northerly winds, and particularly from the north-west or *mistral*. It is not altogether so well sheltered from the north, or rather north-east wind, which, sweeping down the valley of the *Paglion*, is not unfrequently felt during the winter and spring months with considerable severity, but in a degree much inferior, both in frequency and force, to that of the *mistral* in Provence.” p. 29.

This superiority of the climate of Nice is shewn in the luxuriance of its vegetable productions, and the extreme fertility of its soil. As a *winter* climate, indeed, Nice seems to possess every quality of equability and mildness, which are usually esteemed the most essential in the disease to which these observations refer. We fear, however, it will be found, that something more is wanting to constitute Nice, or any other place, an advantageous residence for the consumptive, than the degree of mildness and equability which it seems to possess. At least from our author's account of its diseases, we are led to this conclusion. As a *spring* climate, it seems to fail much more in these requisites.

“Nice is protected from the dreaded *mistral*, but is still subject to some winds, chiefly the north-east, east, and south-east, which, though they do not blow with the violence of those of Provence, nevertheless are sharp and cold. They are particularly frequent during the spring months, and form a strong objection, in my opinion, to the climate, as a spring residence for the consumptive.” p. 36.

Consumption is stated to be a very frequent and fatal disease at Nice; and catarrhal affections are mentioned by the medical men of the place as being their most prevalent complaints. An extract is given from an unpublished work of the

celebrated professor Foderé of Strasbourg, who spent upwards of six years at Nice; from which it appears, that this physician considers Nice as *particularly hostile* to the pulmonary organs. He considers it as beyond doubt, not only that consumption is more prevalent at Nice, and, indeed along the whole of these shores, than in most other situations, but that it is much more rapid in its progress, and fatal in its tendency.

“It seems irrefragably proved, by the experience of our time, that the climate of the shores of the Mediterranean is hurtful to such invalids. I had seen a great number of these cases terminate fatally at Marseilles; and, at that time I considered the very dry and keen winds of that place, as the principal source of evil; but I afterwards found that the warmer, softer, and more humid atmosphere of Nice, was in no respect more favourable to such complaints. Every case of hereditary tubercular phthisis proves fatal, as well at Nice as at Villa Franca, in very early age. In these places consumption is not, as in Switzerland, on the banks of the Soane, and in Alsace, a chronic disease: on the contrary, I have very often seen it terminate in forty days. A physician of the countries just mentioned, would be surprised at the quickness with which one attack of hæmoptysis succeeds another, how readily the tubercles suppurate, and how speedily the lungs are destroyed. The English annually make fresh experience of this melancholy fact, and their burying ground in the *Croix de Marbre* too well testifies its truth.” 48, 49.

Another document, quoted by Dr. Clark, of great value and interest, as illustrating and confirming his opinions of the bad effects of the climate of the northern shores of the Mediterranean, is a Thesis lately published at Edinburgh, by Dr. Sinclair, a navy surgeon, who had served several years in our fleet, in the Mediterranean, and particularly on the coasts of France. This gentleman states consumption, or at least inflammatory affections of the lungs, terminating in suppuration, and ending fatally, to be much more prevalent among our seamen on that station than elsewhere; a difference which he attributes to the extreme variability of the climate. It appears from official documents, quoted by Dr. Sinclair, that,

“During the years 1810, 1811 and 1812, there were admitted into the naval hospitals of the Mediterranean, from the fleet, which contained about thirty thousand men, four hundred and fifty-five cases of phthisis pulmonalis, and one hundred and forty of pneumonia,—making a proportion of one in sixty-five affected with the former complaint, or of one in fifty, when the two diseases are taken together.” 52.

Upon the whole, Dr. Clark seems to have left Nice with a very unfavourable impression of it as a residence for the consumptive.

Villa Franca, situated near Nice, only on the other side of Montalbano, is said to be still milder in point of temperature than Nice. It seems, however, to possess no essential advantages over it.

“This place, I am of opinion, must share the fate of Nice, as to its climate, whatever that may be; for though more sheltered from northerly winds, it has all the other good and bad qualities of the latter place, with the additional disadvantages of possessing few or no accommodations for invalids.” 63.

Pisa is described as being less sheltered by mountain barriers than Nice, as having a more variable climate, and as being considerably colder; the only advantage it is stated to possess over this place is, “that there are good roads leading from it to all parts of Italy, and the invalid may leave it with safety much sooner than he could Nice.”

This reminds us of the well known bon-mot of Johnson, who, when asked of the finest prospect he had seen in Scotland, mentions the *road to London*! It was, therefore, with some surprise, we found our author, in a subsequent part of his book, p. 113, giving *Pisa* a decided superiority over Nice as a winter’s residence for the consumptive. He certainly does not acquaint us with the reasons for this preference. The last place described from personal observation, as a station for consumptive patients, is *Rome*. The following extract will give our readers some idea of the climatorial habitudes of this famous city, as a winter residence for the consumptive.

“The climate of Rome differs considerably from that of Nice and the southern parts of Provence. It is more moist.

The dry cold winds experienced at these places are comparatively little felt at Rome." 70.

"It is chiefly during the spring months, it appears to me, that Rome has the advantage in point of climate, being less liable to the keen cutting winds of the places I have already noticed. This is, however, an important circumstance, as the great difficulty is to find a good spring climate for the consumptive patient; the cold winds, which that season is liable to, I believe over the whole of Europe, being the evil most to be dreaded. At Naples they are particularly complained of. In truth, I believe no place is exempt from them; nor must I be understood as stating Rome to be so: all I wish to be understood is, that Rome is less liable to these winds, than most places I have seen." 71.

As corroborating the opinion the author had been led to form of the favourable nature of the climate of Rome, he quotes, from his own experience, several cases which appear to have been benefited by a residence there; but they are by far too few to permit us to rely much on the evidence they afford. Consumption appears to be a very prevalent disease at Rome; also inflammation of the chest and lungs. Dr. Clark does not appear to have visited *Naples*, but he has been led to consider it as much less favourable for consumptive patients than Rome. He compares it to Nice, in respect to its climate.

After these details respecting the comparative advantages and disadvantages of the various winter residences recommended for the consumptive, Dr. Clark proceeds to examine the question of a *summer residence*. He considers the whole of the south of France and Italy as improper residences during the summer months; and our experience of this disease in the warmer climates of the globe, strongly corroborates this opinion. The following extracts from this part of the work are interesting:

"I believe I shall not err, if I say that the whole of the south of France and Italy are improper for consumptive patients during the summer months, and that the more certainly, the farther the disease is advanced: where hectic fever is formed, and ulceration of the lungs has taken place, a high

temperature certainly accelerates the progress of the disease." 87.

"Though I do not think the observations of Dr. Sinclair are quite conclusive against the good effects of the climate of the Mediterranean and its shores on the generality of cases of pulmonary consumption which occur in England, and are sent abroad to pass the winter months only, they certainly show the impropriety of such patients remaining in that climate during the heats of summer, an observation which I have found corroborated by all the medical men of my acquaintance who have had opportunity of judging.

"It is rather a singular circumstance, that while the English practitioners were sending their consumptive patients to the shores of the Mediterranean, our naval medical officers navigating that sea were sending theirs to England.

"The same injurious effects in hurrying on the fatal termination of consumption, after ulceration had commenced, have been communicated to me by many of my naval friends who had served in the West Indies, as uniformly observed there. And I know that a practice, formerly prevalent in the navy, of sending seamen affected with this disease to that country, in the hope of attaining an alleviation of their complaints, has been of late years abandoned, owing to the representation of the naval medical officers of that country. From these facts, and also from my own observations in the countries themselves, I can have no doubt, nor indeed, do I believe, is there any doubt of the impropriety of consumptive patients remaining in the south of France and Italy during the summer months." 88.

This experience of the evil influence of the high summer heats of Italy is practically acted on by invalids, who are in the habit, it seems, of passing into Switzerland during this season, and returning to Italy in the winter. The places chiefly frequented by them are the neighbourhood of Lausanne and Geneva, which appear to possess no other claim to preference, than the circumstance of being cooler than Italy, and in lying conveniently in the route to that country. The invalids are recommended to reach Switzerland in the middle of June, and to leave it in the end of September. In the con-

cluding portion of Part I., Dr. Clark protests loudly against the practice of sending patients, in a late stage of consumption, abroad. He details several striking and melancholy instances of this cruel measure from the stores of his own experience. The following extract does equal honour to his understanding and his heart; we earnestly call the attention of our readers to it.

“ During my residence on the continent, I have had frequent occasions to remark with surprise, the very advanced stages of the disease in which many of our consumptive patients were sent abroad. This is the more remarkable, as, however medical men may differ about the propriety of sending such patients abroad in the earlier, there surely ought to be no question about its impropriety in the latter stages. For my own part, I have seen enough to convince me, that it is not only a very useless, but often a very cruel thing, to banish such patients from all the comforts of home, and send them forth to undertake a long journey through a foreign country, deprived probably of all they hold dearest to them, and without those thousand nameless comforts by which the watchful care of friends may cheer even the last period of a hopeless disease. The medical man who reflects on the distresses that such patients must be liable to during such a journey, arrested perhaps in their progress by the increase of some of those symptoms which attend the advanced stages of consumption, in very indifferent accommodations probably, and far from any medical advice in which they can confide, will surely long hesitate ere he condemns the fated victim of this remorseless malady to the additional evils of expatriation: and his motives for hesitation will be increased, when he considers how often the unfortunate patient sinks a prey to his disease, long before he reaches the place of his destination; or at best, arrives at it in a much worse condition than when he left England, and doomed, shortly, to add another name to the long and melancholy list of his countrymen that have sought out, with pains and suffering, a distant country, only to gain in it an untimely grave!” 117.

In dwelling on the highly important subject of the effects of foreign climates on consumption, we have carried our ex-

tracts to such a length as to prevent our noticing many other interesting matters in Part I., especially a capital memoir on *Cretinism* and *Goitre*, and a report on the *Malaria Fevers of Rome*. The same circumstance makes us content ourselves with a few extracts only from Part II. This part consists of Notes on several of the Parisian Hospitals, and on the Hospitals and Medical Schools of Lyons, Strasbourg, Turin, Genoa, Pavia, Padua, and Bologna. Of the hospitals of Paris he notices La Charité, St. Louis, Maison des Enfants Malades, Hôpital Necker, Val-de-Grace, Salpetriere et de Clinique Interne. In his notes on La Charité, he confines his remarks chiefly to the practice of Dr. Esquirol, in giving the nux vomica in paralysis. This seems to be a powerful but very dangerous remedy. Several patients have been killed by its use. The following extract from the account of the Maison des Enfants is interesting, and we may add, is very flattering, by way of contrast to the practice in this country.

“In the spring of 1818, small pox was very frequent in Paris, and very fatal; more than half of those admitted into this hospital sometimes dying.

“Strange to say, the heating and sudorific plan was the one adopted at this time in the treatment of these little patients! All those who died were examined, and the appearances observed were inflammation and ulceration of the internal coat of the intestines, pustular eruptions there, or on the surface of the peritoneum; and, in three cases, a false membrane, similar to that of croup, was found lining the whole alimentary canal from the œsophagus to the rectum: in these three cases, the eruption on the surface had never shown itself distinctly. These morbid appearances called the attention of the medical officers to Dr. Broussais’ opinions; the cooling antiphlogistic treatment, with the application of leeches over the abdomen, was adopted in consequence, and the speedy reduction of the mortality soon evinced the superiority of the practice.

“The following statement of the proportion of deaths from this disease is interesting on many accounts; and taken in conjunction with the circumstance just detailed, cannot fail to make a strong impression on the mind of the English reader.

“ Small Pox Cases admitted into the Hospital des Enfants Malades, in 1816, 1817 and 1818.

	Cured.	Died.	Total.
1816	42	53	95
1817 [From Jan. to Aug.]	51	37	88
1818	117	102	219
	<hr/>	<hr/>	<hr/>
	210	192	402
	<hr/>	<hr/>	<hr/>

Page 150.

In his account of L'Hôpital Necker he dwells chiefly on the discovery of Dr. Laennec, on which we shall say nothing, having said so much in our last number. We are happy to see our opinion of the utility of Dr. Laennec's diagnostics corroborated by that of so intelligent a practitioner as Dr. Clark.

In his notes on the Val-de-Grace, he gives an account of the doctrines and practice of Dr. Broussais, an exposition of which we expect soon to lay before our readers in detail. We cannot help remarking, however, at present, that, like most theorists, this eminent physician is too exclusive and confined in his mode of explaining morbid action.

In almost every disease that affects the human body, Dr. B. can see nothing but inflammation of the mucous membrane of the stomach and intestines.

Dr. Clark appears to entertain but an indifferent opinion of the *practice* followed in the Parisian hospitals.

“ The practice appeared to me particularly inert: ptisanes pectorales, eau gommée, lavement emollient, &c. were among the most active prescriptions, and few escaped without some one or two of these. 'Tis sickening to an English physician, in visiting the hospitals of Paris, to hear nothing but these eternal ptisans ordered for every patient, let the disease be chronic or acute. It may be said they are harmless at any rate, and may satisfy the patient that something is doing. But surely the latter consideration is little necessary in hospital practice, and I am inclined to think that the plan is often worse than harmless, as it operates, in some degree, on the mind of the physician, and leaves an impression that he has done something, when, in truth, he has done nothing. In this

way, perhaps, it prevents the employment of more active means when they are actually requisite." 170.

He seems to us to account pretty well for this state of things.

"Generally speaking, the physicians in the hospitals of Paris, as appears to me, have too many patients to attend. Though it was summer when I last visited them, and there were extremely few fevers and acute diseases, yet Dr. Fouquier, of La Charité, had one hundred and four patients under his own charge, with one assistant only. Dr. Recamier, of the Hotel Dieu, had one hundred and ten, and the physicians of St. Louis had a still larger proportion.

"The fatal cases are generally examined after death. Dr. Fouquier told me, that for twelve years that he had been physician to La Charité, no patient had died without being examined.

"It is allowed, that post-mortem examinations, when properly conducted, form one of the principal means of enabling the physician to acquire an intimate knowledge of the nature of disease, and to lead to those principles which are to guide him in the treatment of it. But the knowledge derived from these examinations, with whatever care they may be performed, when taken by itself, is but of little real utility. If the previous history of the disease be not accurately known, little is to be expected from them; and it is a deficiency of this knowledge that appears to me the principal circumstance which has rendered these examinations at the hospitals of Paris of so little real value. I have attended not a few of these examinations, and seen the surprise excited at some appearances, which, had the morbid phenomena during the course of the disease been carefully observed, must have been known at least, if they could not have been remedied; and I have also had occasion to remark the difference that sometimes occurred between the physician and assistants with respect to previous symptoms. All this convinced me how truly useful these researches might have proved, but for the want of information relative to the history and progress of the disease. This circumstance, however, certainly cannot be sufficiently at-

tended to, where the physician has so many patients under his care." 130, 131.

An interesting and full account is given of the hospitals at Lyons. It appears that the antiphlogistic mode of treating febrile disease, is making gradual progress here also, and not before it is time, since Dr. Clark informs us, that *even now*, four-fifths of the practitioners in this city are Brunonists! We quote the following statement in justice to our continental brethren.

"In this gentleman, Dr. Desgaultières, I found a very enlightened physician, who had had much experience, both in the army and in private and hospital practice; and, in the present unsettled state of the opinions of medical men about the cause of fever, his may be stated here with propriety. I do this the more readily, as it appeared to me that his opinion was formed, not from any theoretical notions, but from careful and extensive observation, and because it approximates so closely to that which the great increase of pathological researches has led so many British practitioners of late to adopt. His idea is, that fever always depends upon local affection, irritation or inflammation of some organ, or even an unusual accumulation of blood, congestion, in some viscus, without absolute inflammation. He is far from limiting such local affection to the gastro-enteritic system with Dr. Broussais; yet his conviction is, that fever invariably depends upon, and has its origin in some local affection, and that from this we ought to set out in our explanation of its phenomena." 183.

Dr. Clark's account of the medical school of Strashbourg is extremely meagre. Its prosperity seems on the decline; "a few years ago, the number of medical students amounted to from four to five hundred; at present, there are only about two hundred, and from thirty to forty only take their degrees annually." The Italian schools seem somewhat more flourishing. Among these, Bologna ranks as one of the first.

"Medicine at Bologna is cultivated with assiduity, and the professors are men of established reputation. Tommassini, professor of Medicine, is well known as the author of an excellent work on yellow fever, and several others of less note. He is one of the great supporters of the new Italian, or con-

tra-stimulant doctrine, as it is called; and which, in practice at least, is directly opposed to the system of Brown, which for a long time was advocated warmly in Italy. The latter, it is well known, considered almost all diseases as arising from debility, and requiring stimulants, while the present doctrine, 'Nuova Dottrina Medica Italiana' looks upon most diseases as arising from increased action, and requiring depriments—contra-stimulants. The medical school of Bologna may be considered at the head of this new doctrine, and the professors its most able and zealous advocates." 193.

Dr. Clark gives a full detail of the plan of medical study prescribed by the university, which seems very judicious. The course extends to four years.

"The mode of examination of candidates for the different degrees seems very judicious, and is considerably different from that followed in our medical schools. Five professors constitute a quorum; and at the examination of a candidate, each submits to him twenty different subjects, taken from his own particular course of instruction. He draws one of these by lot, and is forthwith examined upon it, whatever it may be. At the termination of the examination, the professors give their votes secretly, and the number of these obtained, and the mode in which they are obtained by the candidates, constitute three different modifications of the same degree, which are expressed in the diplomas.

"If they obtain two-thirds of the votes they are declared *approved*; if the whole, they are declared *unanimously approved*, a pleni voti; and to express a still further praise, they are declared *honourably approved*,—*approvati con lodi*. Those who are rejected cannot present themselves till after another year's study.

"I was told that the plan of study just described is about to be changed, by an order from Rome." 199.

"There are altogether about two hundred medical students at the university of Bologna; but of these, a small proportion only aspire to the higher ranks of the profession. Many, I found, were merely preparing themselves for being apothecaries, and many to practise the lower branches of surgery—'Bassa Chirurgia,'—a class in the profession unknown among

us, and holding a rank something similar to the 'officiers de santé,' in France.

"This system of sending forth a set of half educated men, who, no doubt, as in other countries, overstep in their practice the limits prescribed to them, is, with justice, complained of by the regularly educated members of the profession in Italy. The Germans are blamed for introducing this practice. A system more calculated, indeed, to bring the profession into disrepute, cannot well be imagined; and it may be one, among several other causes, of the very different rank in society which the medical men of Italy hold, compared with those in England. But a still stronger reason may be found, I apprehend, in the system of gratuitous medical education in use in the former country. The students attend the lectures and hospitals almost for nothing, and the graduation fees are very trifling. The annual salary of the medical professors themselves, amounts only to about 150*l*. The facility thus afforded to medical students of acquiring their profession, on easier terms than the lowest mechanic can his trade, so common on the continent, and of which they boast, though it may have some advantages, is, upon the whole, I am inclined to believe, injurious to the profession." 196.

"*Padua*. The university of Padua at present holds a deservedly high character in Italy, as a medical school. Within the last few years, the number of students has considerably increased, and the present building being found too small, the emperor, on his late visit to Italy, gave orders for the erection of a new one, on a more extensive scale. The plan of medical studies in the university of Padua is good. It extends to five years; and two courses of lectures are delivered in each session." 204.

The following extract respecting Pavia will be interesting to all who venerate, and who does not venerate, the name of the great man, with whose name that of his city must be eternally associated in the memory of the votary of medicine.

"Every thing about the medical school of Pavia is very perfect, and the musea, class rooms, &c. all arranged and fitted up in a very superior style; yet I was surprised to find every thing on so small a scale at a university, which, at one time

collected so great a concourse of students from all parts of Europe, and boasted of some of the first medical professors in the world.

“As a medical school, Pavia is now much fallen in estimation. The number of medical students of all classes in the session of 1817 and 1818, I was told, amounted to nearly four hundred; and that about forty annually took the higher degrees of the profession. The wards of the general and clinical hospital were in good order, but small. We must not, however, forget, that the town is but small, containing only about 20,000 inhabitants. This a circumstance which must always have been against Pavia as a school of practical surgery. It ought, however, to add not a little to the merit of the venerable Scarpa, whose works have added so largely to the stock of surgical knowledge, and to whom this university is so much indebted.

“Scarpa is now the director of the medical department of the university; the plan of medical study at which, I shall not mention, as he informed me they are about to change it. Scarpa's sight has failed him much, yet he appears to enjoy good health, and a flow of spirits and cheerfulness not often met with at his age. Although in a great measure retired from the exercise of his profession, he has lost nothing of his enthusiasm for it. In his library, I found all the best English medical works. He complained much of the very tedious manner in which he received publications from our country, being obliged frequently to get them round by Germany.

“Whoever visits this great man will, in addition to the pleasure and information derived from his animated conversation, find in his house a collection of some of the finest paintings by the first Italian masters; for Scarpa is an admirer and an encourager of the fine, as well as the useful arts.” 210.

The whole of the establishments in the Sardinian dominions seem to wither beneath the deadly sway of an ignorant and despotic government. We shall conclude this article by the following most horrible account of the lunatic asylum, Casa de' Pazzi, of Turin.

“My expectations of this establishment were not raised ery high, from the state in which I found that which I have

just mentioned, and still less from some conversation I had with one of the medical professors of the university the evening preceding my visit, during which I perceived evidently a wish that I should not see it; but I never could have anticipated the dreadful spectacle of tortured humanity which was here exhibited, and of which any description I can give must come far short of the impression it made upon my mind. I shall simply state the situation of the wards and patients as I found them at the time of my visit.

“The part of the hospital we were first taken to, consisted of small rooms, similar to those generally met with in such institutions, but I was disappointed to find these were not for the poor patients, but for those who paid a certain sum for being kept. The first of these that was opened presented to us the wretched prisoner perfectly naked, and chained down to his bed by both wrists. He had raised himself up in his bed, as far as his chains admitted, by which movement he had thrown off the single coverlet that had been cast over him. He had no shirt; his legs, apparently red and swollen from cold, were drawn up under the corner of the bed cover, which lay over a small part of his body; he was pale and emaciated; he uttered not a word. In short, a human being in so wretched a state I had never before seen; but I was soon to witness others in a state still more horrible.

“We were next conducted into a ward where thirty beds were huddled together, on most of which lay a poor wretch chained by one or more limbs to the bedstead; for to each corner of these was attached a massy iron chain, with a clasp of the same materials and strength at the extremity, for admitting the wrists and ankle; and, according as the keeper judged necessary, one or more of these were applied. Some were polished, and bright as silver, from constant use. I imagined these were their most unruly patients, but was told that this was by no means the case.

“To these we were next led; and on the unbolting of the door of a large cell, the scene that presented itself almost exceeds belief. The spectacle of the poor wretches, naked, or covered only by some straw, chained down hand and foot to their bedsteads, the clanking of their chains, the dreadful vo-

cifications they set up at the sight of him who riveted these chains, and still more, the horror excited by such a spectacle, no terms are strong enough to depict! I had read and heard of chains and other means of torture for subduing, [irritating] the unfortunate maniac; I had even seen such singly chained to the wall by the neck, like an infuriated and dangerous beast; but a den like this, crowded and crammed with human beings, chained down, without a rag of covering, struggling to raise their heads, and exhibiting their emaciated and galled limbs from the heap of straw that had been thrown upon them, was a scene I never expected to witness, and which I hope in God I may never witness again! After visiting similar establishments in Paris, and some in our own country, where chains have been laid aside, and a mild system of treatment adopted, with a success which, setting humanity out of the question, more than warrants its continuance, I was ill prepared to meet such a dreadful contrast on crossing the Alps. In this cell there were twelve men, three of whom only were allowed any thing more than straw to cover them. Some I was told had been confined there for many months. On approaching them, they exhibited their chained limbs with the most earnest entreaties for liberation. One man had two chains on one arm. In this case, the space between the iron clasps was red, swollen, and ulcerated, and the mortification, which, in all probability was to follow, would soon render chains unnecessary for him. Others had their limbs galled, but not in such a degree as that described. In one instance only, in the whole hospital, did I observe any thing introduced between the iron ring and the limb. The rest of the men's wards were similar to that I first noticed.

“ From the men's we were led into the women's department, which was in the higher part of the house, and which, in every respect, we found similar to that we had just left; the beds huddled closely together, chains always ready, many applied, and most of the beds occupied; for, whether to save trouble, or from the poor creatures having no clothes but the coverlet that was thrown over them, almost every one was in bed. Here, as below, was also a cell, where straw afforded

the only covering, where the chains were more heavily applied, and where the state of furious desperation to which the wretched victims were driven, was expressed in terms equally violent and still more affecting. One of these tortured women held up her arm, which was raw and had been bleeding, from the iron clasp having worked its way into the flesh!

“Such is the dreadful state of this house, which contains one hundred and eighty males and ninety-seven females; of whom one-third, the keeper told me, were kept *constantly* chained. From the same source I learned, that the annual number of deaths, and this I apprehend is the principal way in which this house gets rid of its inhabitants, sometimes amounted to eighty, nearly one-third of the whole; that they *had* been as few as thirty; and that the average was fifty, nearly one-fifth—a mortality I believe unequalled in any institution of the kind in any country.

“With respect to the murderous receptacle for the deranged, I know not what relief to propose; nothing but a perfect change, I fear, can remedy the state it is in; perhaps it would even be for the benefit of humanity, that the same dreadful remedy were applied to it as was formerly applied to the Hotel Dieu of Paris; and could it rise from its ashes in *Piedmont*, as that hospital did in France, he who applied the torch would deserve to be ranked among the benefactors of his country and mankind. In its present state, it is an infamous disgrace to the government under which it exists: I say government, because, in a despotic one, such as that of *Piedmont*, where private opinion dares not be expressed, the government alone is responsible for the state of all such institutions. But these things are beneath the notice of a king of *Sardinia*, who, by the way, is particularly bound to visit *La Carità* in person; but he prefers a deputy, who again, I fear, employs a substitute in imitation of his magnanimous sovereign. The royal family of France are not ashamed, in their visits to the departments, to look into the hospitals, and even to administer consolation at the bed-side of their subjects; and the emperor of *Austria*, in his late journeys, visited the hospitals, as well as other public institutions: but such is the horror of an hospital at the court of *Sardinia*, that a physician who entered one, is not suffered

to cross the precincts of the palace, lest he should carry the seeds of infection among the royal family!" 223.

After this ample exhibition of interesting specimens of Dr. Clark's volume, in presenting which to our readers, we have endeavoured to minister to their gratification, while we offered a fair view of the work itself, it is hardly necessary for us formally to express our opinion on its merits. These certainly are very considerable, but they are far from being without alloy. The greatest fault of the work is its brevity. Of some very interesting scenes, so very little is said, that we cannot easily acquit the author of negligence, or want of curiosity, during his visits to them. He, indeed, tells us in his preface, that he professes to give us only "detached notes;" and seems to consider, that we ought to be thankful for what he gives us. We ARE thankful; yet, we repeat, that we would have been more satisfied had he given us *no* information respecting some of the places, than we are by the meagre morsels which he has vouchsafed to give us from the rich and overflowing granaries to whose half-opened portals he conducts us. We trust, however, that before Dr. Clark finally revisits his native country,* he will be enabled to fulfil the hopes expressed in the preface, of filling up the outline which he has chalked out in the present essay.

"At all events," he says, "this essay will be useful to myself, as a sort of text book for future observations of a like nature, and as a basis whereon, I hope, more extensive opportunities may enable me to rear a superstructure more creditable to myself, more commensurate with the importance of the subject, and more worthy the attention of the public."

* Dr. Clark, we understand, is at present residing at Rome, of which city he is preparing a Medical Topography, which cannot fail to be highly interesting.

Pharmacologia; or, the History of Medicinal Substances. By JOHN AYRTON PARIS, M. D. F. L. S. Third edition. 8vo. London, 1820.

[From the London Medical Repository, for May, 1820.]

WE hail the appearance of a third edition of this work as a proof of the greater attention to the study of the *materia medica* and pharmacy than was usual a few years ago. Notwithstanding the reveries of the old chemical physicians, respecting the existence of one single universal medicine which should cure all constitutional diseases, had long passed away like the baseless fabric of a vision, yet there still existed in the minds of the majority of the profession an undue propensity to rely upon the exhibition of mercury, the modern panacea, iron, antimony, bark, ardent spirits, and a *few* other potent remedies, in different doses, rather than upon the skilful use of a more varied *materia medica*; a preference which was in some measure cherished by the legal necessity imposed on the great majority of actual practitioners of furnishing medicines to their patients, and their consequent wish to make as small a stock as possible, and especially of perishable articles, serve their purpose.

It may, perhaps, be said, that this preference of a paucity of drugs was also owing, in part, to the want of sufficient chemical knowledge as to the compounding of a more varied list, from an apprehension of introducing substances which might have the effect of neutralizing or even altering the intended action of the main ingredient; especially as the old dispensaries afforded but a slender guide in this respect. Pharmaceutical chemistry has, however, received so much illustration from the labours of the German and French apothecaries, that the compatibility or incompatibility of substances with one another in the same formula are well ascertained.

Still, however, there remained a want of a scientific treatise in our vulgar tongue respecting the mode of compounding medical formulæ, or the art of prescribing; and it is to supply this want that Dr. Paris has principally directed his attention. Not that he neglects the other parts of pharmacy, usually giv-

ing under each article of the London pharmacopœia, to which alone he very properly confines his attention, and the materia medica and preparations of which he has incorporated into a single alphabetical series, its sensible qualities, its chemical composition, its relative solubility in different solvents, and the proportions in which it should be mixed or combined with different bodies, in order to produce suspension or saturation; the substances that ought not be mixed with it, the most eligible forms in which it can be exhibited, the proper doses to secure specific effects, these medicinal uses and effects, the officinal preparations of the simple substances, and, lastly, the usual adulterations. As he notices all these in a most distinct and satisfactory manner, it may easily be conceived, that this work forms a most excellent manual for the practitioner of medicine; and from a perusal of it, we can recommend it in the strongest terms, not only to the junior members of the profession, but even to the most experienced, who will find in this volume many new observations.

The work is preceded by an historical introduction, the substance of four lectures delivered by the learned author to the Royal College of Physicians, on the Philosophy of the Materia Medica; in which he investigates the causes that have in different ages operated in producing the revolutions that characterize the history of medicinal substances; namely, superstition, credulity, scepticism, false theories, devotion to authority, and established routine, in which he gives an account of the mischievous interference of the college of Paris, in regard to the use of antimony, the assigning to art that which is the effect of unassisted nature, the ambiguity of nomenclature, the progress of botanical science, the application and misapplication of chemical philosophy, the influence of climate and season on diseases, as well as on the properties and operations of their remedies, the fraudulent adulteration of medicines, the unseasonable collection of plants, and the obscurity which has attended the operation of compound medicines.

We perfectly agree with Dr. Paris, in his observations on the frequent changes of names which the colleges have adopted from the philosophical chemists. His words are:—

“ I am unwilling to join in the commendations which have

been so liberally bestowed upon our chemical nomenclature; nay, I am disposed to consider it as a matter of regret, that the names of our medicinal compounds should have any relation to their chemical composition; for, in the present unsettled state of this science, such a language must necessarily convey theory instead of truth, and opinions rather than facts; in short, it places us at the mercy and disposal of every new hypothesis, which may lay our boasted fabric in ruins, and, in its place, raise another superstructure, equally frail in its materials, and ephemeral in its duration. Thus CORROSIVE SUBLIMATE was a *muriate* of mercury, or an *oxymuriate*, until Sir H. Davy established his new theory of chlorine, and then it became a *bichloride*. At some future period chlorine will be found to be a compound, and then it must have another name: for the same reason the term CALOMEL is surely to be preferred to *submuriate* or *chloride*. TARTARIZED ANTIMONY again has been called by our nomenclatural reformers, the *tartrate of antimony and potass*; but is it a triple compound? GAY LUSAC thinks not, and considers it as a combination in which *cream of tartar* acts the part of a simple acid. The French, in their new *codex*, are still more extravagant in their application of chemical nomenclature; thus, the subcarbonate of potass is called by them the *sub-deuto-carbonas potassii*. The first part of this quadruple name indicates the comparative quantity of acid in the salt, the second that of oxygen contained in the base, the third announces the acid, and the fourth the basis of the base."

Although we conceive it of little consequence, yet we cannot but imagine that Dr. Paris is in an error in attributing the origin of alchemical notions to the Arabs. In our opinion, those notions first originated in Egypt, among the Christian clergy: this appears evident, from the high ecclesiastical rank of the principal Greek authors, several of whom were anterior to the age of Arabian learning, and also from the religious mysteriousness of their writings; whereas the Arab authors, as Geber, are remarkable for their clearness; and, although they give processes for the production of apparent gold and silver, yet acknowledge the imperfection of them, and discuss a number of theories, by the reduction of which to practice it

had been judged possible to produce perfect metals; so that nothing can be more erroneous than deducing the etymology of the word gibberish, from Geber, as stated by Dr. Paris, from Dr. Johnson.

Another trifling mistake in the history of chemistry refers to the name of the great Coryphæus of the chemists, the redoubted Paracelsus. Dr. P. informs us, that he termed himself Philippus Theophrastus Bombastus Paracelsus de Hohenheim; but although here are names enough even for a person of no ordinary abilities, yet one is omitted, and the order of the others disturbed; his name, as stated by himself, being Aureolis Phil. Par. Theoph. Bomb. de H. As the activity and violence of this man were the means of rousing the profession from the study of the old Greek physicians to that of nature, and the origin of our modern improvements, so we ought not to detract either from his name or his merit.

In the second part of this introduction, Dr. Paris investigates the objects to be obtained, and the resources which are furnished, by medicinal combination, according to the following order:—

“*Object 1.* To promote the action of the basis, by combining it with substances which are of the same nature, *i. e.* which are individually capable of producing the same effects, but with less energy than when in combination with each other; or by combining the basis with substances of a different nature, and which do not exert any chemical influence upon it, but are found by experience, or inferred by analogy, to be capable of rendering the stomach, or system, more susceptible of its action.

“*2.* To correct the operation of the basis, by obviating any unpleasant effects it might be likely to occasion, and which would pervert its intended action, and defeat the objects of its exhibition.

“*3.* To obtain the joint operation of two or more medicines, which have different powers, and which are required to obviate different symptoms, or to answer different indications.

“*4.* To obtain a new and active remedy not afforded by any simple substance, either by combining medicines which excite different actions in the stomach and system, in conse-

quence of which, new or modified results are produced; or by combining substances which have the property of acting chemically upon each other, the results of which are the formation of new compounds, or the decomposition of the original constituents, and the development of their more active elements; and also by combining substances, between which no other chemical change is induced than a diminution or increase in the solubilities of the principles in which their medicinal virtues reside.

“ 5. To afford such a form as may be efficacious, agreeable, or convenient.”

In that great and important question, Whether any chemical proofs of the presence of white arsenic, short of its actual reduction to the state of metal, can be depended upon, or ought to be received as evidence in the courts of criminal law? Dr. Paris says,

“ That after a full experimental investigation of the subject, and after an impartial review of all the facts which bear upon the question, the author feels no hesitation in declaring it to be his conviction, that white arsenic may be detected without any fear of fallacy by a proper application of tests, and that the contrary opinion is entirely founded in error.”

Now, although we may accede to this proposition, when the examiner is a professed student of chemistry, and versed in its manipulations, we cannot by any means agree to it in cases when the examination is made by a person who has not bestowed continual attention on that science, but who has merely studied it slightly as a subordinate branch of medicine, or, perhaps, of pharmacy; and, therefore, as the increasing number of books on medical jurisprudence will bring, of necessity, the subject of poisoning continually before the public eye, and we may, of course, expect a great increase of that horrid crime, and a greater skill in exhibiting deleterious substances; so we fear, on the other hand, that a desire of appearing to possess more knowledge of chemistry than they really do, will induce many to rest satisfied with slight indications, to give decisive evidence in doubtful cases, and thus bring many innocent persons to a shameful death. We therefore think, contrary to Dr. P., that as it is better that ten guilty

persons should escape than a single person, or, to speak more properly, family, as all are implicated in the shame, should suffer, so nothing but an actual procuring of the deleterious article should be regarded by a medical person as warranting his affirming the party to be poisoned by it; and that the use of pretended tests should be regarded only as preliminary experiments to point out the method of obtaining the arsenic, or other poisonous substance.

In the examination of elaterium, a substance whose action on the human body is still more violent than arsenic, Dr. Paris has discovered a new vegetable principle. The active principle of this plant is lodged only in the juice around the seeds, and in so small a quantity, that Dr. Clutterbuck (MEDICAL REPOSITORY, Vol. XII. No. 67,) obtained only six grains from forty cucumbers; and even of this fecula, or feculence, as Dr. Paris proposes to call it, although an eighth part of a grain will purge violently, yet not more than one-tenth of this virulent substance possesses any active virtues. As Dr. P.'s experiments are new, and a very excellent supplement to Dr. Clutterbuck's paper, we shall insert them at full length:—

“Experiments.—Series the First.

“A. Ten grains of elaterium, obtained from a respectable chemist, and having all the sensible properties which indicated it to be genuine, were digested for twenty-four hours, with distilled water, at a temperature far below that of boiling; *four grains* only were dissolved.

“B. The solution was intensely bitter, of a brownish yellow colour, and was not in the least disturbed by alcohol, although a solution of *iodine* produced a blue colour; the solution, therefore, contained no gum, and only *slight traces* of starch.

“C. The solution, after standing twenty-four hours, yielded a *pellicle* of insoluble matter, which, when burnt, appeared to resemble *gluten*.

“D. The six grains which were insoluble in water were treated, for forty-eight hours, with alcohol, of the specific gravity .817, at 66° of Fahrenheit; a green solution was obtained, but by slow evaporation *only half a grain* of solid green matter was procured. The insoluble residue obstinately adhered

to and coated the filtre, like a varnish, and completely defended the mass from the action of the alcohol: it is probable that it consisted principally of *fecula*.

“ Experiments.—Series the Second.

“ E. Ten grains of elaterium, from the same sample, were treated with alcohol, of the specific gravity .817, at 66° Fahrenheit, for twenty-four hours; upon being filtered, and the residuum washed with successive portions of alcohol, the elaterium was found to have lost only 1.6 of a grain. The high specific gravity of the alcohol in this experiment was important; had it been lower, different results would have been produced.

“ F. The alcoholic solution, obtained in the last experiment, was of a most brilliant and beautiful green colour, resembling that of the oil of cajeput, but brighter: upon slowly evaporating it, 1.2 grains of solid green matter were obtained.

“ G. The solid green matter of the last experiment was treated with boiling distilled water, when a minute portion was thus dissolved, and a solution of a most intensely bitter taste, and of brownish yellow colour, resulted.

“ H. The residue, insoluble in water, was inflammable, burning with smoke, and an aromatic odour, not in the least bitter; it was soluble in alkalies, and was again precipitated from them unchanged in colour; it formed, with pure alcohol, a beautiful tincture, which yielded an odour of a very nauseous kind, but of very little flavour, and which gave a precipitate with water; it was soft, and of considerable specific gravity, sinking rapidly in water; circumstances which distinguish it from common resin; in very minute quantities it purges. It appears to be the element in which *all* the powers of the elaterium are concentrated, and which have been denominated *elatin*.

“ I. The residuum, insoluble in alcohol, weighing 8.4 grains, (Exp. E.) was boiled in double distilled water, when 5.9 grains were dissolved.

“ J. The above solution was copiously precipitated *blue*, by a solution of *iodine*, and was scarcely disturbed by the *persulphate of iron*.

“K. The part insoluble, both in alcohol and water, which was left after experiment I, amounted to 2.5 grains; it burnt like wood, and was insoluble in alkalies.”

From these experiments, Dr. Paris expresses the chemical composition of elaterium in the following manner:—

I. {	F.	Water,4
	B.	Extractive,	2.6
	B. D. J.	Fecula,	2.8
	C.	Gluten,5
	K.	Woody matter,	2.5
	H.	<i>Elatin</i> ,	} 1.2
	G.	Bitter principle	
				10

The bitter principle in elaterium is very distinct from its extractive matter; the solution obtained in Exp. G. being diluted and swallowed, produced only an increase of appetite; and the solution B. produced no effect whatever.

Dr. Paris mentions the most generally used nostrums with the due abhorrence of medicines whose preparation, being unknown, are, of course, liable to be prepared differently by different persons, however well-intentioned they may be, for want of some certain standard; but we are surprised to see one excepted from this merited proscription, at p. 260, whose name we omit, not wishing to be instrumental in giving it publicity, where he not only informs us that the medicine “*is admirably adapted for domestic use*,” but also obligingly gives the names and residences of the proprietor and his confidential agent. We know not how to account for this departure from his accustomed tone respecting these arcana, having too high an opinion of Dr. Paris to suspect, even for a moment, any interested motives for this lenity, and, what is more, praise.

He mentions, in his preface, Mr. Barry’s patent extracts, prepared *in vacuo*, and conceives, *à priori*, that the results must be more active than those obtained in the ordinary way. Respecting this subject we may observe, that having examined some of these extracts, we find that of belladonna inferior in smell to the common; and, also, that the extract of senna be-

came speedily covered with mould, and those of papaver and taraxacum are beginning to grow mouldy, although not the least mouldiness is to be found on the common extracts of the same plants, which were obtained before the others, and all the pots have been kept in the same drawer. It would appear, therefore, that the heat used in preparing these extracts, *in vacuo*, is not sufficient to destroy the vegetative power of the seeds or sporules of the fungi adhering to the plants.

BIOGRAPHY.

FOR THE ECLECTIC REPERTORY.

Biographical Notice of Dr. La Roche.

DIED on the 6th of June last, in the sixty-fifth year of his age, RENE LA ROCHE, M. D. Member of the Royal Society of Medicine of Madrid, Honorary Member of the Medical Society of New Orleans, &c., formerly of the Island of St. Domingo, and for many years a highly respected practitioner of this city.

In recording the death of this truly amiable and learned physician, but whose extreme reserve and modesty had in a great measure limited his sphere of usefulness and consequently his reputation to the circle of his countrymen, we pause for a moment at the reflection which naturally suggests itself, of the wide difference between the fame that survives the man of practical utility alone, and that brilliancy which ever surrounds the funeral car of the medical professor. The society in which we live, or perhaps it would be more correct to say, mankind at large, so few of whom are competent to decide on the merits of medical men, are naturally led to forget the claims of the useful and laborious practitioner, amidst the bursts of eloquence from the medical rostrum or the genius and talents displayed in the writings of a Rush, a Barton, or a

Wistar. The fame of such teachers, and of such writers, is undoubtedly deserved and should be cherished with reverence,—but when we do so, do we perform all our duty? Does not the following their precepts for the relief of suffering man, constitute the greatest claims of all physicians to the gratitude of society? It is on this account that the memories of those friends of mankind throughout the world, who leaving aside the fascinating paths of literary and ambitious preferment, have sacrificed the enjoyments of their whole lives to the tedious and painful labours of suffering humanity, should not perish with their mortal remains. Justice and gratitude require that we should not allow the examples of the benefactors of the human race to sink into the oblivion of the tomb; but on the contrary, that we should treasure up their remembrance in our hearts and hold them up as models to the rising generation.

If a long life devoted to the relief of the poor and sick, if the practice of those virtues, which conciliated the esteem and won the affections of all who knew him, can entitle one man to the gratitude of his fellow men, our departed friend carried with him to another and a better world, the regrets of the worthy and the just. But those who have survived him—those whom his friendship and intimacy had honoured while a sojourner in this valley of tears, have one more duty to fulfil;—a duty not more required by departed worth, than by the society he so long contributed to comfort by his example and by his life.

Réné La Roche was born of respectable parents in the Island of St. Domingo in the year 1755. According to the customs of the wealthy inhabitants of the country he was sent at an early period to France, to receive his collegiate education. Having completed his classical studies, he felt desirous of becoming a disciple of Hippocrates, and persevering in his intention, he soon inscribed his name among the students of the then celebrated university of Montpellier. As a medical school, Montpellier at that period had no rival, except that of Edinburgh, whether by the number of its students or by the talents and acknowledged fame of its professors. Lamure, Barthez, Leroy, Vigaroux, Broussonet, &c. were his teachers; and from these fathers of the art, he received, with his medi-

cal instruction, that persevering spirit of observation, which distinguish him through life. Under these masters he could not fail becoming ardently attached to the profession of his choice; and after going through the usual trials with credit to himself, he received his diploma in 1779.

The zeal and ambition so natural to his age, urged him to commence the practice of his profession as soon as he returned from Europe in 1780. Possessed of solid instruction, well grounded in the principles of the science of medicine, and endowed with a strong and reflecting mind, his claims to public patronage were not overlooked;—an extensive circle of patients soon bestowed upon the youthful practitioner their full confidence, and in the course of a few years, fortune, often blind, but sometimes clear sighted, had showered upon him her choicest favours,—but alas! how futile are her gifts, how delusive her pleasures. The monster revolution showed its hydra head in this favoured and remote spot, and war, havoc, and ruin desolated another Eden, a land where happiness had fixed her abode. The misguided sons of Africa, in the fervor to recover their freedom, gave way to every excess, and fire and the sword became the instruments of destruction to proprietors and property. It is too fatally engraven in the hearts of many who may chance to peruse these lines, that a general massacre of all who could not defend or save themselves, soon swept from the face of this devoted island every one of its white inhabitants. But in the midst of this horrific transaction, while blood was streaming from every pore, some latent sparks of humanity still existed in the hearts of these deluded men. Gratitude, though suppressed, was yet alive;—it impelled them not only to preserve, but even to defend the lives of those physicians, who in fulfilling the duties of their profession, had used the language of kindness and protection. Dr. La Roche and some of his surviving colleagues and friends now in this city, were of this number.

Having thus escaped the fury of these men of blood, Dr. La Roche with many of his acquaintances reached the friendly shores of this country in 1793, where they experienced that liberal hospitality which distinguishes the American character. The fatal epidemic which at that time desolated this conti-

ment, soon brought into notice the French physicians, and the success that attended their mode of treatment justified the attention of the inhabitants of Philadelphia. And without detracting from the high claims of the practising physicians of that period, we may be allowed to state, that their colleagues from St. Domingo were equally fortunate in the treatment of the yellow fever. Let it not be urged that they derived from a previous acquaintance with the disease, the experience necessary to counteract its effects, for we know from the most unequivocal and honourable testimony, that until they had reached the American continent, they were utter strangers to this devastating form of disease. It would, however, savour of partiality were we not to concede, that the bilious remittent form of fever which prevails so often in the West Indies, had furnished them with results by which they were guided in the treatment of our yellow fever; and how judiciously, can be attested by numerous surviving and grateful individuals.

From that period our revered friend became a resident of this city, and as it continued to be the place of refuge of many of his countrymen, he soon found himself fully employed in his profession. Among the victims of that unhappy period his philanthropy and his benevolence found ample scope for their exertion. Nor were his useful talents confined to that very respectable portion of our inhabitants. During twenty-seven years of laborious practice, he was often called to visit American as well as French families; and though a stranger to the language as well as to the habits of the former, he gained their confidence and commanded their respect.

Dr. La Roche had always enjoyed a feeble state of health, which the labours of his profession by no means contributed to fortify. Perhaps also the vicissitudes of our climate had aggravated it; for he was subject during the latter years of his life to attacks of lumbago, which though not very severe, required much care and attention. He was just recovering from one of these attacks, and had been enabled for a few days to walk out, when he was thrown by accident in the street on the 2d of June, and received a severe contusion on the parietal bone. Conveyed senseless to his house, he never recovered to their full extent, the exercise of his faculties, but after various

gleams of returning reason, and of proportionate hope to the crowd of friends that thronged his door, closed, on the 4th day of the accident, his valuable life. It would be useless to add, that the illness of so respected a member of the profession, soon collected around him some of the most eminent of his colleagues; and that their assiduity and anxious attention, on this melancholy occasion, could not be exceeded;—but it was in vain that they exerted their skill.

In the various relations of man, of husband, and of father, no one fulfilled his duties with more kindness. The expression of his feelings under a reserved, and at times a pensive deportment, conveyed to strangers perhaps an appearance of insensibility,—but those who knew him will bear testimony to the warmth as well as to the uniformity of his attachments. The mildness of his disposition, his forbearance, his constant serenity of mind, and his passibleness, were, perhaps, unexampled. As a physician, his humanity and disinterestedness knew no bounds. He has left, besides his respectable widow, an only son, who had just obtained the medical honours of the university of Pennsylvania; and who, in the remembrance of his fathers reputation and benevolence, will find ample encouragement to tread the arduous path of medical science.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

METEOROLOGICAL OBSERVATIONS, &c.

STATE of the weather at Philadelphia during the first six months of 1820.

JANUARY.

Thermometer—Lowest at 8 A. M. 11. 1st day of the month.
Highest at 3 P. M. 40. 7th and 28 days.
Mean 30.

Winds southerly—some heavy rains and several snows—measles continue—whooping-cough common.

FEBRUARY.

Thermometer—Lowest at 8 A. M. 14. 1st day of the month.
Highest at 3 P. M. 69. 24th day.
Mean 35.

Winds, north-east and south-west—heavy rains—deep snow—thunder, lighting, and rain on the 16th—measles on the decline.

MARCH.

Thermometer—Lowest at 8 A. M. 30. 7th day of the month.
Highest at 3 P. M. 70. 25th day.
Mean 50.

Westerly winds—clear weather—measles continue—ice islands met with in the Atlantic in this month.

APRIL.

Thermometer—Lowest at 8 A. M. 29. 3d day of the month.
Highest at 3 P. M. 87. 20th day.
Mean 52.

Winds, north-east and south-west—snow on the 1st—clear, and some very warm weather—little rain—measles nearly extinct—rheumatism more common than usual—scarlatina frequent.

MAY.

Thermometer—Lowest at 8 A. M. 55. 16th, 17th, 18th days.
Highest at 3 P. M. 79. 1st and 23d days.
Mean 60.

Westerly winds prevalent—clear until the end of the month, then heavy rain—scarlatina continues, and has proved mortal in several instances in this and last month.

JUNE.

Thermometer—Lowest at 8 A. M. 61. 12th day of month.

Highest at 3 P. M. 91. 30th day.

Mean 70. 70

Winds, north-east and south-west—generally clear weather—some heavy rains—warm season—several plentiful showers—though the drought was considerable—most abundant production of the fruits of the earth—a bilious remittent, often terminating in typhoid symptoms occurred in the last month and continued in this—not unusually mortal—chiefly confined to the black people—no small-pox.



The publication of our present number having been unavoidably delayed, we are enabled to insert the following interesting communication from Poulson's American Daily Advertiser.

A comparison of the mean heat of the two summers of 1819 and 1820, that is to say, from June 1st to August 15th in each year. The observations being made on the same thermometer, in the same situation and exposure, at a house in north Eighth street, in this city.—At sun-rise in the open air, and at 3 P. M. in a room on the second story of the house, with the window sash all fully raised, and the shutters half closed or bowed, as it is called.

	Sun-rise, out.	3 P. M. within.	Mean of both.
1819.			
June, - - - - -	65.9	79.4	72.65
July, - - - - -	68.4	80.8	74.6
Aug. 1 to 15, - - -	74	84.86	79.43
	<hr/>	<hr/>	<hr/>
Mean of the 76 days,	68.5	81	74.75
	<hr/>	<hr/>	<hr/>
1820.			
June, - - - - -	63.36	78.1	70.06
July, - - - - -	71.4	81.6	76.5
Aug. 1 to 15, - - -	70.7	81.3	76
	<hr/>	<hr/>	<hr/>
Mean of the 76 days,	68	80.1	73.8
	<hr/>	<hr/>	<hr/>

By the above statement it appears, that the mean heat of the 76 days, from June 1 to August 15, has been lower in

1820 than in 1819, nearly one degree, on an average, of sunrise and 3 P. M.; from which it might be inferred, so far as heat is an agent, that the health of our city should be better or more free from sudden mortality this season than the last.

Comparisons between one year and another ought always to be made on the same thermometer, in the same situations, otherwise the comparison will be of no use, as there is a difference of from one to five degrees at 3 P. M. in hot weather, between one situation and exposure and another, though both may be in the shade.

C. E.

College of Physicians of Philadelphia, July 4, 1820.

The following officers of the College were duly elected.

President.—Doctor Thomas Parke.

Vice President.—Doctor Samuel P. Griffitts.

Censors.—Doctors William Currie; Henry Neill; Thomas T. Hewson; Edwin A. Atlee.

Treasurer.—Doctor Thomas C. James.

Secretary.—Doctor Samuel Emlen.

PRIZE MEDALS

Offered by the Humane Society of Philadelphia.

THE discoveries which have attended the recent labours of physiologists, have encouraged the Managers of the Humane Society to believe, that continued attention to the consideration of suspended animation, may be productive of benefit; they are therefore induced once more to offer,

For the best practical dissertation on the means of restoring persons apparently dead from drowning, *a gold medal of the value of three hundred dollars.*

For the second best dissertation, *a gold medal of the value of two hundred dollars.*

The dissertations may be written in the English, French, or Latin language, accompanied with a sealed paper, containing the author's name and place of residence.

They must be sent to the secretary of the Society, on or before the first day of the first month, January, 1822.

They shall be submitted to the judgment and decision of the College of Physicians of Philadelphia; and those which are not successful, shall be returned to their authors.

By order of the Managers.

J. WILSON, Sec'y.

JOSEPH CRUKSHANK, Pres.

Philadelphia, 6mo. (June) 14, 1820.

Obituary.

DIED, in Philadelphia, June 6, 1820, Doctor RENE LA ROCHE, aged sixty-four years.

DIED, at Montreal, on the 11th instant, after a lingering illness, which he bore with fortitude and christian resignation, Mr. FREDERICK PURSH, the celebrated botanist, aged forty-six years. He was a native of Russia. As a scientific and practical botanist his abilities were of the first order. Among his various publications, the *American Flora*, which he published in London in 1814, is esteemed by the learned both of Europe and America, as a standard work of the first authority.

He came to Canada for the purpose of exploring the country, and to collect materials for a Canadian Flora. This work, if his health had been spared, and from his known intention of exerting his utmost capacity and means to render it complete, useful, and splendid, would have been a great acquisition to those who feel interested in the productions of Canada. As a man, he was candid, liberal, and unassuming. His remains were followed to the grave by the members of the Horticultural Society, as a tribute of respect to his memory, as a distinguished member.

DIED, in England, on the 19th of June at his house, Springgrove, near Hounslow, the venerable President of the Royal Society, the Right Hon. Sir JOSEPH BANKS, G. C. B., &c. &c. The loss to science by the demise of this excellent man and liberal patron will be long and severely felt. Sir Joseph had been for a long time labouring under a most distressing illness; for some years he had been deprived of the use of his lower extremities, and rendered so feeble as to be lifted from his room to his carriage. He possessed a princely fortune, of which he assigned a large portion to the encouragement of science, particularly natural history, private and public charities, and domestic hospitality.

London Medical Society, Bolt Court.

[From the London Medical Repository, for April, 1820.]

The following is the list of officers and council for the year 1820.

President.—Dr. Clutterbuck.

Vice Presidents.—Dr. Uwins; Dr. Blegborough; W. Norris, Esq.; John Abernethy, Esq.

Treasurer.—John Andree, Esq.

Librarian.—Dr. Hancock.

Secretaries.—Dr. Uwins; Thomas Calloway, Esq.

Secretary for Foreign Correspondence.—Henry Blegborough, Esq.

Council.—Dr. Babington; Dr. Walshman; Dr. Merriman; Dr. Pinckard; Dr. Copland; Dr. Blicke; Dr. Furnival; Dr. Ley; Mr. Johnson; Mr. Hooper; Mr. Lowdell; Mr. Bartlett; Mr. Leatham; Mr. Leese; Mr. Suttleffe; Mr. Drysdale; Mr. Pettigrew; T. Bryant; Mr. Dunlap; Mr. Hilliard; Mr. Harris; Mr. Shephard; Mr. Tarratt; Mr. Burrows; Mr. Ollier; Mr. Wigan; Mr. Morley; Mr. Good; Mr. Wray; Mr. Brown.

Chairmen of the Council.—Dr. Walshman; T. J. Pettigrew, Esq.

Library Committee.—Dr. Walshman; Dr. Copland; Dr. Furnival; E. Suttleffe, Esq.; T. J. Pettigrew, Esq.

To deliver the Anniversary Oration, 1821.—F. Calloway, Esq.

Registrar and Sub-Librarian.—James Field.

Hunterian Society.

The first annual meeting of this Society was held at the King's Head, Poultry, on the 10th of February; Sir William Blizard in the chair.

The following gentlemen are elected officers for the ensuing year:—

President.—Sir William Blizard, F. R. S.

Vice Presidents.—James Hamilton, M. D.; John Meyer, M. D.; Benjamin Robinson, M. D.; Lewis Leese, Esq.

Secretaries.—John T. Conquest, M. D. F. L. S.; William Cooke, Esq.

Council.—Thomas Bell, Esq., F. L. S.; Isaac Buxton, M. D.; Thomas Callaway, Esq.; W. D. Cordel, Esq.; R. Dunglison, Esq.; John Dunston, Esq., F. L. S.; H. Greenwood, Esq.; William Kingdon, Esq.; Charles Meeres, Esq.; James Miles, Esq.; Benjamin Pierce, M. D.; Henry R. Salmon, Esq.

Communications on Medical and Physical topics, addressed to the Secretaries, No. 10, St Mary Axe, will be thankfully received.

THE
ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

VOL. X.

OCTOBER, 1820.

No. IV.

SELECTED PAPERS.

On the Efficacy and Fallacy of New Remedies. By Dr.
KINGLAKE.

[From the London Medical and Physical Journal, for July, 1820.]

THE materia medica has been already carried to a great extent. The proposal of new remedies has in all ages been received with indulgence, and in numerous instances an intemperate earnestness has been shown for their adoption. Although the medicinal substances that have been progressively admitted and catalogued as possessing sufficient remedial powers to entitle them to such an adoption, have been vastly extensive and various, yet there is still manifested an anxious desire for augmenting the number. This arises from the strong recommendation with which new remedies are usually introduced, not abiding the test of experience. They are very generally found, on trial, to be at least wanting in the efficacy ascribed to them, if not altogether unworthy of any degree of reasonable dependence. This has proved a source of serious injury and disappointment in the practice of medicine. The instruments of medicine are of as much importance to the medical practitioner as suitable tools are to the expert mechanic:

in neither case can the desired object be effected without their respective aid. The indications for the use of medicine are numerous, and are required to be fulfilled by appropriate means. Much may always be accomplished by dietetic and other domestic regulations with almost infallible certainty. Positive and negative directions in medical practice for obtaining given results, are amongst the most valuable resources of the healing art. It often happens, however, with the best-poised consideration of all the relative circumstances that may be adduced for and against a medical view of a disease, and all the benefits accruing from this knowledge, that the relief which may be urgently required cannot be procured by any known medicinal influence. The aid that may be wanted is always sufficiently evident, but how it is to be afforded is the difficulty. *Hoc opus, hic labor est.* In this solicitude for applying an adequate remedy, every instance that presents of furnishing superior medicinal powers is eagerly embraced, and with hopes and expectations that oftener disappoint than realize the promised advantage.

It is less to be wondered that new medicines should fail in verifying the high character at first given of their efficacy, than that, under the usual precipitate and unguarded commendation of their powers, they should be ultimately admitted into the lists of the *materia medica*.

It is quite notorious, that many of the officinal articles adopted and sanctioned by the late revisions of the most respectable Pharmacopœias, are destitute of any certain powers that could be beneficially employed under any known circumstance of disease. The instances in which many of them have been at all availing are so vague and indefinite, that better authority would seem to be necessary to warrant their unqualified adoption. It is easier to object to the inefficacy or fallacy of medicinal substances, than to substitute what would possess more precise and efficient virtue; yet that should not be regarded as resting-ground which really has no sure foundation. It would be better to complain of the insufficiency of an adopted medicine, than to connect it with powers and capabilities which, in the moment of trial, it will be found not to possess. The precise influence attributed to various medicines being taken too

much on trust, prevents those researches which are necessary for furnishing that description of influence.

In the *materia medica*, as in other departments of medical science, knowledge cannot be stationary; it must be progressive. What is known at this time may, at a future period, under farther combinations and illustrations, be understood so differently, as greatly to extend the intrinsic worth of the acquisition. It is not in the nature of things that medicinal substances should uniformly produce a similar effect in all constitutions, or even in the same individual, at all times. The variations, the discrepancies, the unfavourable circumstances occurring on one occasion and not on another, must necessarily lead to widely different results. Hence the frequent fallacy of reputed remedies; and hence the physical impossibility of strict uniformity in medicinal influence, and the utter absurdity of confiding in specific nostrums.

Beyond the sphere of the first passages medicinal influence is precarious, and cannot be reasonably regarded as affording any infallible ground on which to estimate its ulterior actions. The sympathies naturally subsisting between the alimentary canal and the system at large, are powerful enough to connect them together, by ties of medicinal as well as of nutritive influence. Whether the various impressions produced by different medicinal substances on the stomach and intestines, are transmitted to portions of the system possessing a corresponding susceptibility to be affected; or whether the substance exerts a direct material agency, by finding admission into the system, is a question involved in too much perplexity for the present state of physiological knowledge satisfactorily to solve. But this uncertainty offers no objection to the fact, that certain parts, whether of visceral, tabular, fibrous, or reticulated arrangements, are as it were specifically affected by the peculiar influence of given agents. The diversified actions of the various substances of the *materia medica* depend on this principle; and it is in the precise adaptation of the agent to the peculiar susceptibility of being affected by it, that consists the greater or less efficacy of all remedies in accomplishing their curative purposes.

It is most clear, that no medicine can infallibly produce a

given effect in the variable state of the animal economy.— Whatever harmony or aptitude for a certain influence may exist at one period, will be found disconcerted and inoperative at another; so that nothing but approximation to the precise effect contemplated can generally be expected. If the morbid condition requiring to be remedied be founded in either excessive or deficient excitement, in an habitual or a distempered state of action, in too much or too little susceptibility for being affected by medicinal influence, the appropriate means for correcting either or all of these different morbid states will be more or less efficient, according to inherent circumstances of organization, or to accidental conditions of prevailing excitability. It is only by reiterated trials, close observation, and accumulated results, that a just conclusion can be formed of the real power of medicinal substances, and to what extent their efficacy and fallacy in relieving and curing disease may be safely estimated and allowed.

Digitalis, *uva ursi*, and acetate of lead, have of late been more particularly regarded as possessing remedial powers in all stages of phthisis pulmonalis. That each and all of them, with many other articles that might be enumerated, are capable of exerting considerable influence in allaying, and perhaps, in certain circumstances, even in subduing, the diseased action existing in that ailment, will not be denied; but that either of them is entitled to the reputation of being specifically efficacious in curing that malady, is a position that may be reasonably doubted. Experience has not hitherto justified the early character respectively assigned to these remedies. They have, no doubt, when assiduously and discriminatively given, severally afforded at least palliative, if not curative, benefit in the recoverable states of pulmonary consumption. But, to suppose that the salutary influence occasionally exerted by them should uniformly take place, would be to imagine a condition of vital similarity in all relative circumstances that but rarely occurs. The deviations from a given standard are so constant, and often so wide, as to preclude all reasonable expectation that the same effect should necessarily arise. Approaches towards it may, and indeed frequently are made, and in the incompleteness of the resemblance is found the want of specific efficacy.

The failures are as explicable as the successes, and equally rest on the principle of dissimilarity, more or less influential on medicinal agency obtaining in the excitable conditions of vital power. It is therefore wrong to reject a medicine because it is not uniformly available for the curative objects in view. If it be so in certain instances, it deserves to be retained; and trials should be patiently instituted, to ascertain when, and in what probable manner, its most salutary effects appear to be produced. When a curative indication in the treatment of disease cannot be fulfilled by a medicine usually employed for that purpose, it is possible the deficiency may be supplied by an agent possessing analogous qualities; so that what was not attainable by one mode of power, may be effected by another kindred one, or by a conjunction of their similar virtues.— Thus, digitalis will often be more efficacious when united to acetate of lead, and these two when in triple connexion with uva ursi, than when given separately. The practice of conjoining substances in medicinal composition is vindicable on the ground of the insufficiency of single agents to effect the desired object. The power that may be wanting cannot always be supplied by an augmented dose of the same article, but by the combined influence of various substances, each sustaining and extending the other's efficacy.

It is precipitate and unphilosophical to propose any remedy as capable of acting with undeviating sameness of power. The requisites for such an agency have no existence in the animal economy; it is therefore a chimera which adequate trials refute, and never establish. The highest character that can be justly given to a new medicine is, that it will prove less fallacious than its predecessor in reputation; that it will not so often disappoint expectation; but it must be at the same time allowed, that it can act by no means uniformly in the dissimilar and variable circumstances in which it will have to operate in different constitutions. A medicine should not be abandoned because it cannot effect impossibilities; but should be retained and upheld as an useful acquisition, when it is capable of fulfilling the curative intention with which it is given oftener than any other known medicine could have done. In this estimate of comparative worth, digitalis, cerussa acetata, and uva ursi,

deserve to be regarded as valuable medicines; but not of a description to be considered as acting with either precisely similar or infallible efficacy.

The reputation to which the muriates of lime and barytes have severally aspired, for effectually relieving and curing scrofulous disease, has not been warranted by experience. The object to be effected by their influence is too difficult to admit of being realized in a way that would entitle either these or any other agents to much reliance. To correct and repair morbid derangements of the glandular system, and to reinstate that portion of the general frame in a healthy condition, may be anxiously desired, but cannot be readily accomplished. Carbonate of iron, cinchona, soda, cicuta, and arsenic, may respectively lay fair claim to considerable efficacy in scrofulous diseases; but, if any pretension be made to a direct specific or a uniformly curative power, the endeavour to obtain it would prove fallacious. Much reasonable expectation might be indulged as to the salutary influence of these different substances; but the various and uncertain conditions in which they must have to operate, when subjected to trial, must necessarily render the results always too doubtful to admit of unqualified confidence.

Eau medicinale, colchicum, white hellebore, and alkalies, have severally obtained a reputation more or less ephemeral, for relieving and curing gouty irritation. That each of these remedies may so affect the excitability of the system, as in some measure to counteract the painful sensation of arthritic disease, cannot be reasonably disputed; but, that either can be regarded as possessing specific powers in overcoming that description of ailment, is inadmissible, on the same principle that we would deny a similar claim to any sort of medicine that could be proposed. It is the extreme uncertainty of this effect that should refer the pretensions of all new remedies to the test of actual observation, guarded by a due consideration of the improbability of the same physical circumstances occurring in the different individuals who may be subjected to their influence. Unvarying uniformity is not known in any of the departments of nature; general resemblances mark the characters of kindred substances; but this analogy is too loose for intrinsic

and unexceptionable identity. It is in this inherent dissimilarity in the animal economy amidst an apparent sameness, inducing a persuasion that no influential difference could exist, that consists the ground on which medical remedies produce their dissimilar effects under apparently similar circumstances. In this innate organic diversity will be found an adequate explanation of the uncertainty, the insufficiency, and of what would occasionally appear to be the capricious or arbitrary effects of medicines. To know the susceptibilities of vital action in all their original and acquired power, would be to understand the almost infinite variety of effect that may result from the same medicinal agents. The discordancy which thus presents itself to the observant practitioner, would suggest to him the importance of scrupulous attention to the power of medicine in different persons, that he might, at length, be enabled to come to some general conclusion that would be steadily applicable to the medical purposes of the healing art.

It is hardly conceivable, that direct specific and permanent efficacy will ever be produced by any particular agent, however high its just reputation may be, and with whatever encomiums its introduction to public notice may be accompanied. The deficiencies of one agent should be supplied by the analogous quality of another; and, in this mode, sufficient remedial power may be gathered from sources that yield similar efficacy. Like all other knowledge, that of medicine is progressive; and, were that which is worth retaining preferred to the attraction of novelty, and were more assiduity employed in simplifying and classing medicinal influence, it would be soon seen that useful practical acquirements are of slow growth, and can only result from rigid analytical examination and unsparing critical inquiry. Severe and patient observation is necessary, to be enabled correctly to appreciate the value of medicinal efficacy. The existing properties, whether chemical, mechanical, or medicinal, should be investigated and understood, so that a reasonable inference may be drawn of what would be the probable effect of such an agent. Close attention will also detect the constitutional circumstances that may appear to be most favourable to a given medicinal effect. When these relative peculiarities are sufficiently known, much reasonable confidence

may be indulged in the usual and probable result. Although unbounded reliance on medicinal agency may savour of empiric confidence, yet a certain extent of expectation may be justly held, without fear of disappointment. When remedies, in a very large majority of instances, on a scale of adequate trial, shall have vindicated their claim to specific efficacy, an authority sufficiently competent is afforded for depending on a continuance of such power. It is by this tenure that all medicines of reputed efficacy are possessed and used. The most valuable remedies known in medical practice have no higher title to credit than the customary manner in which they operate beneficially. Every new medicinal substance should be brought to the test of experiment. It should be cautiously ascertained what it can effect, and in what pretension it is likely to fail. When practical proof has been obtained on its real *bona fide* capability, then the visionary notions of specific and infallible powers in certain states of disease should be corrected and restrained by the consideration, that the sameness of condition which is necessary to insure an unvarying effect does not present; and that, therefore, such inevitable uniformity has no existence in any sort of medicinal operation.

Writers of the highest authority have been unjustly regarded as case-makers, in some of the details which they have given of medicinal efficacy. The results of trials supposed to be fully sufficient to ascertain all that was necessary to be known, have been published of the remedial powers of various medicines. It is indeed to be regretted, that these accounts have not been verified, in the observation of others, to the extent proposed by the authors of them; yet it would be illiberal to suppose, that they have not been communicated, in most instances, under a clear persuasion of their truth and validity. The incorrectness, if any, on these occasions, would appear to be a too-exclusive reference to the cases in which success had occurred; so that the view given, instead of being a general, was a selected statement of the favourable issues of the trials that had been instituted. This is a partiality into which an anxious desire to advance the practical improvement of medicine is apt to betray an unguarded inquiry after facts. The statement indeed contains facts; but they are too insulated, and

form too desultory a connexion with the whole that should be brought under consideration, to admit of their being at all conclusive of the general efficacy intended to be deduced from them. On these occasions it would be better to state the facts, and to cite unreservedly the instances of failure as well as of success. All the trials should be mentioned, and all the attending circumstances should be minutely detailed, which would collectively present a correct practical view, not only of what had actually occurred, but of what would be likely invariably to happen in all subsequent inquiries.

The accounts which have been lately published of the curative efficacy of belladonna, in that afflictingly-painful complaint the tic douloureux; of hydrocyanic or prussic acid, in pulmonary affections; and of colchicum, in gouty disease; cannot be wholly destitute of truth. They have, no doubt, respectively operated in a salutary manner; but yet neither of them can be implicitly relied on in the several ailments in which they are held to be curative. All medicinal efficacy should be measured and weighed most scrupulously; the *pro et contra* should be impartially estimated; and the real worth of the evidence be decided by all that is known concerning it. Impatient examinations and hasty conclusions are almost necessarily erroneous; whilst patient inquiry, and cautious induction from indisputable facts, will as inevitably conduce to ascertain and establish truth.

Taunton, May 7th, 1820.

On a New Method of Preparing Pharmaceutical Extracts. By
JOHN T. BARRY. Communicated by DR. MARCET.

[From the Medico-Chirurgical Transactions—Vol. X. Part I.]

IT has not failed to be matter of regret with medical men, that many of the pharmaceutical extracts, although prepared from materials of the same quality, vary considerably in their

efficacy, in consequence of having suffered partial destruction in the process of inspissation. This has justly been attributed to the excess of heat employed in their preparation; and it seems also to be generally allowed, that the presence of atmospheric air, by affording oxygen during the process, is injurious. There are some other circumstances attending the common mode of evaporation, which likewise operate as minor causes of uncertainty in their efficacy, such as the degree of agitation or stirring bestowed on the fluid; the surface exposed, and the depth of it in the evaporating vessel; but especially the quantity operated on at a single process, as on this must depend the duration of the destructive causes. Considering all these as concurring circumstances, we are forced to admit, that it is scarcely possible to produce two different parcels of an extract by the common method, which shall even possess the same efficiency. Hence, a disparity in the effects of these medicines does frequently occur; and independently of the evil arising from the use of inferior materials, this disparity is the ground of just objection to the use of extracts, from the natural reluctance a medical man feels to administer remedies, the strength of which he has no standard to ascertain; and this objection is rather increased by the circumstance of our being unable to separate, by any purifying process, the efficacious part of an extract from the part which has become inert. Solution, crystallization, or precipitation, means which yield us superior advantages in the preparation of saline and metallic substances, are here of no avail. It has therefore long been a desirable object to possess means less exceptionable than those hitherto in use, for the preparation of this class of medicines; and conceiving that a method of my own, which has been carried into execution on a large scale, and is free from the chief objections to the common process, is possessed of some practical utility, I have thought the subject would have sufficient weight to entitle the present communication to the attention of this Society.

By the present nomenclature of pharmacy, the term *extract** is applied to the inspissated residua of vegetable solu-

* Except in the instance of the *extractum elaterii*.

tions; some of these solutions are imperfect, and the residua obtained from them must in every case be of a very compound nature. These solutions are natural, being the expressed juices of plants; or artificial, consisting of decoctions or infusions of vegetable substances. The menstruum in general is water; but of the latter description some few are alcoholic mixtures. Most commonly the menstruum is intended to extract all the active ingredients; in a few cases it is designed to dissolve but certain parts of them; and with respect to the inspissated juices, the utility which they should possess, is that of presenting in a concentrated and durable form, the medicinal virtues of the recent juices. Our business then in forming an extract is to reduce to a solid state the medicinal part of the solution; to withdraw the menstruum, and preserve in the residuum the same efficacy in kind and degree which the solution possessed, except such as depends on matter, which, in a chemical sense, is volatile. And some plan which shall accomplish this is what we seek; perhaps it is attainable: if however perfection is not to be expected, still, if a process which presents the means of producing the extract of uniform quality can be offered, it will secure what is certainly our principal object.

In order to prevent the injurious effects of heat on medicinal extracts, I proposed some years since, in conversation on the subject, to conduct the evaporation in *vacuo*. The low temperature at which fluids are known to boil in exhausted vessels, together with the supposed advantage this plan possesses of preventing the access of oxygen, led me to expect decidedly good effects from its execution.

The subject selected for the first experiment was a solution of opium. The next was some juice of hemlock. Both of these were evaporated in a water-bath, the vessels having been exhausted by an air-pump; and the products exhibited appearances, which left no doubt of the superiority of this method for preparing extracts. Accordingly I decided on carrying the project into execution at some future day; and on shewing a sketch of the necessary apparatus to my partner William Allen, I had the satisfaction of finding that his opinion supported my own. Soon afterwards it was submitted to my friend Dr. Marcet, who not only gave it his approbation, but so strenu-

ously advised its adoption, that it was concluded to have the vessels constructed on a large scale. It is remarkable, that only a short time before the large apparatus was made, this gentleman received from Professor Jaenisch, of Moscow, a communication on the same subject, proposing to prepare extracts in vacuo. Dr. Marcet thought it proper, on that occasion, to advise my not being informed of the plan of the Moscow Professor, till my own apparatus was finished.

To persons acquainted with the process of Edward C. Howard for refining sugar, and for which he took out a patent, it may appear that I have but small claim to invention, for proposing a very similar process to obtain extracts. The two kinds of apparatus are however materially different. I employ no pump, or machinery, but produce the exhaustion wholly by steam; and the vacuum is maintained in my vessels by immersion of the whole in water.* The difficulty with which air is prevented from finding its way into exhausted vessels, seems the chief impediment to conducting evaporation in vacuo.— This difficulty, by the simple expedient just mentioned, is completely remedied. The apparatus being thus rendered easy of management and less expensive, appeared likely to be employed in some other large manufactures, such as sugar-refining and colour-making; and on that account I have been induced to take out a patent for it. It is, however, to be recollected by this Society, that I have declined having a patent for its pharmaceutical products. Chemists, desirous of inspissating extracts in vacuo, are therefore at liberty to do it in any apparatus differing from that which has been made the subject of my patent; and thus these substances may continue the objects of fair competition as to quality and price.

Notwithstanding the disadvantageous circumstances which generally attend inspissation in the open air, I have little doubt that many of the extracts might be obtained in that way, of very superior, though perhaps not of uniform strength, by complying with certain conditions, which are, however, suited

* This idea, original with myself, was some time since publicly made use of for a particular object; and lest I should be supposed to have borrowed it from the invention alluded to, I may here take leave to say, that its adoption, in that instance, was the result of my own advice.

only to the scale of philosophical experiment, or to the consumption of an individual practitioner. But it is well known that the trouble attendant on these preparations, renders it much more convenient to purchase than to make them. Hence it has become the business of the druggist to supply extracts; and making them on a scale proportionate to his demand, he is obliged, in order to produce rapid evaporation, to subject them to a temperature, little, if at all, short of 212° , one that seems to be decidedly injurious. This happens even when the steam-bath or the water-bath is employed; and extracts so prepared must still be considered as having been partially subjected to the contingencies before-mentioned. In fact, it may be stated on good authority, that extract of rhubarb, for instance, when inspissated in vacuo, is fully twice as effective as when boiled down in the open air over steam.

Any improved method then for extract to become really useful must be adapted to the large scale of manufacture; and this is one of the features of the method which I propose; for it admits of being carried into effect to almost any extent, and with a proportionate diminution of cost.

As perhaps it will be satisfactory to the Society to form for themselves a judgment upon the fitness of the method I have adopted, and of the apparatus by which it is carried into effect, I propose here to describe them both. The mode of procuring the vacuum, and the manner of maintaining it, have already been mentioned. The degree of exhaustion is of course judged of by the column which is supported in the mercurial gage; and I think it will excite some surprise, when it is stated, that although no pump is employed, yet that column is often at a height of 28 inches during rapid ebullition. In fact, it is common to operate with a column not two inches less than the barometer of the day, and at such times the temperature of the boiling fluid is below 100° Fahrenheit, often at 95° ; and I am satisfied, that by certain improvements this temperature may be reduced to less than 90° in the ordinary process of manufacture. The vessels employed in the apparatus first put up, were two, the one of cast iron, polished on its inner surface, serving as the evaporating pan, and situated in a water-bath, may be called *a still*. The head of it leads into the second ves-

sel, which is a large copper sphere, about three times as large as the other, and surrounded at pleasure by cold water; it may be called *the receiver*. In the pipe which connects these two is a large stop-cock, by which their communication with each other can be suspended. The manner of setting it to work is this: the juice or infusion is introduced through a large opening into the polished iron still, which is then closed, made air-tight, and covered with water. The stop-cock which leads to the receiver is also shut. In order to produce the vacuum, steam is allowed to rush through the copper sphere, until it has expelled all the air, for which five minutes is commonly sufficient: this is known to be effected by the steam issuing uncondensed. At that instant the copper sphere is closed, and the steam shut off, and then cold water admitted upon its external surface. The vacuum thus produced in the copper sphere, which contained about four-fifths of the air of the whole apparatus, is now partially transferred to the still by opening the intermediate stop-cock. Thus four-fifths of the air in the still rushes into the sphere, and the stop-cock being shut again, a second exhaustion is effected by steam in the same way as the first was: after which a momentary communication is again allowed between the iron still and the receiver; by this means four-fifths of the air remaining after the former exhaustion is expelled. These exhaustions repeated five or six times, are usually found sufficient to raise the mercurial column to the height before-mentioned. The water-bath in which the iron still is immersed, is now to be heated until the fluid that is to be inspissated begins to boil, which is known by inspection through a window in the apparatus, made by fastening on air-tight a piece of very strong glass; and the temperature, at which the boiling is kept up, is determined by a thermometer. Ebullition is continued until the fluid is inspissated to the proper degree of consistence, which also is tolerably judged of by its appearance through the glass window. I prefer taking for a single operation, as much juice or infusion as will keep the apparatus employed for nearly the whole day. When inspissated sufficiently,* the residuum

* It frequently happens that the extract has become too stiff; but this is easily remedied by gently warming it, and kneading it with sufficient water.

which we denominate extract, is taken out and is fit for use.

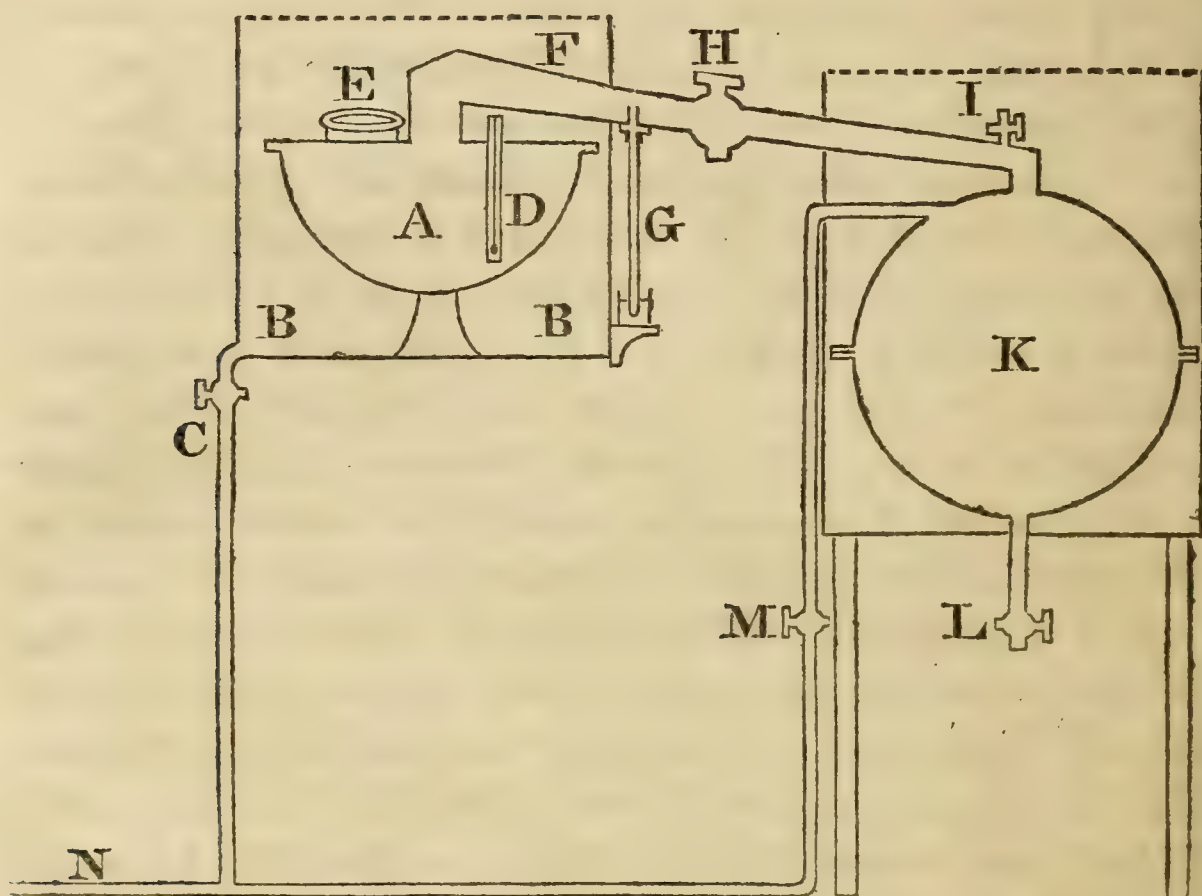
On comparing the temperature employed for ebullition in vacuo with that generally in use, we are led to expect a considerable difference in the sensible qualities of extracts. With some this is the case in a very striking degree. The extract of hemlock is an example; its taste and smell are remarkably different, as is the colour both of the soluble and feculent parts; it also possesses an extraordinary degree of tenacity, a property which is not generally found in the common extract; and it abounds so much with crystallized matter as to produce a gritty sensation when rubbed between the fingers. The extract of belladonna contains a large quantity of crystals of some kind of salt; but I have not succeeded in procuring either of these in a separate state, so as to give them an examination. In some attempts to obtain them pure, and more particularly to discover whether morphia (the new vegetable alkali) was traceable in the narcotic extracts, I became acquainted with the singular fact, that phosphoric acid in a soluble state is to be found in all the extracts. On further extending the investigation, it was ascertained that this acid, besides that portion of it which exists as phosphate of lime, is contained in a vast variety of vegetables. It would be rather foreign to the subject of this paper to enumerate the substances that were tried, but I may just mention, that all those vegetables which are cultivated seem to contain phosphoric salt in great abundance.

The extract of taraxacum is another remarkable instance of the difference in the sensible qualities of these preparations; instead of being sweet to the taste and high-coloured, like that prepared in the common way, it is bitter and extremely pale when fresh made; its taste much resembles that of the plant itself. With respect to the strength of extracts made in vacuo, I have not yet gained sufficient information to be able to present a view of the relative proportions which they bear to the common extracts; but I have been informed by several medical friends, who have given them a trial, that they find them materially stronger.* Perhaps some gentlemen will consider the

* I deem it almost indispensable to state, that the increased activity of extracts when prepared by this new method, makes it imperative on those who compound

subject sufficiently deserving of investigation, to collect such a statement of cases as will enable them to present to the Society, the relative doses. I shall be glad to offer for the acceptance of such, specimens of any kind, which they may be inclined to make use of.

*Plough Court, Lombard street,
25th of 5th month (May), 1820.*



- A Iron still, or evaporating-pan.
- B Water-bath.
- C Steam-pipe to heat this bath.
- D Thermometer indicating the internal temperature.
- E One of the covers, in which is a glass aperture.
- F Pipe leading from the still to the receiver.
- G Mercurial column to measure the degree of exhaustion.
- H Stop-cock.
- I Cock for admission of air.
- K Receiver in the refrigerating vessel.
- L Cock for drawing off the condensed water.
- M Cock for admitting steam when the air is to be expelled.
- N Pipe leading from the steam-boiler.

medicines to continue using the old kind, except where extracts prepared in vacuo are specially prescribed.

Observations on the Diseases of the Teeth. By THOMAS BELL,
Esq. F. L. S. Communicated by Mr. TRAVERS.

[From the Medico-Chirurgical Transactions—Vol. X. Part I.]

AT a period when every department of the profession, and almost every disease has its particular votaries, it is much to be regretted that so little has been done towards affording a correct history of the diseases of the Teeth, and the parts connected with them. Some of them remain even yet undescribed, and others, though occasionally adverted to, are but little understood. Yet when the importance of the organs themselves both to our comfort and health is considered, as well as the consequences which frequently result from the neglect of them, whatever tends to the elucidation of their diseases, or points out the means of their prevention or cure, will not, it is presumed, be thought unworthy the notice of the profession.

The admission of the fact that the teeth possess vitality is *essential*, before we can possibly account for some of their morbid appearances. I believe that this theory is now generally received amongst those physiologists who have paid any attention to the subject; there are however still some men of high and deserved celebrity who hold the contrary opinion. Mr. Lawrence considers “the vascularity of the teeth, a doctrine refuted by every circumstance in the formation, structure and diseases of these organs;”* and Monsieur Blainville† distinguishes the tooth itself (“*la vraie dent*”) by the words “*la partie morte.*” This hypothesis was also very lately taught in one of the first medical schools in the world.

I shall not now enter into the numerous arguments which may be adduced in proof of the position that the teeth are, strictly speaking, *organized*; although I am convinced that this would not be difficult, either on the ground of analogy or experiment. I shall content myself with mentioning two simple and decisive facts, which neither require illustration, nor will allow of controversy.

* Blumenbach’s Comp. Anat. translated by Lawrence, p. 67, note (O).

† Nouveau Dictionnaire d’Histoire Naturelle, &c. Art. “Dents.”

CASE.

Mr. ———, a medical gentleman in London, requested me to extract the second molaris, on the right side of the lower jaw. He had for some time past been suffering the most excruciating pain, which still continued; and which, although not exactly of the nature of common toothach arising from caries, was at times so severe as to be almost intolerable. He stated, that some months previously he had taken cold, and had the usual symptoms of inflammation in the membrane of a tooth; since which time he had rarely had any interval of ease. The alveolar process and gum had undergone absorption, so that the fangs were in part exposed; and although there was not any external appearance of disease in the tooth, yet as it was productive of so much pain and had become so loose as to be totally useless, I immediately determined on its extraction, which occasioned as much pain as usually attends the operation. I then sawed the crown of the tooth through transversely, in order to ascertain if the state of the membrane would afford any satisfactory explanation of the symptoms, and found a cavity formed in the bony substance of the tooth, communicating with the natural cavity, and filled with pus. There was not the slightest appearance of that softness and discoloration which essentially characterize caries; on the contrary the texture of the surrounding bone remained unchanged, and possessed a remarkably firm and healthy appearance. There is but one way of accounting for this. Here was a distinct case of abscess formed in the bone of a tooth; the membrane had been inflamed, suppuration followed, and the pressure of the pus occasioned *absorption*. How could this take place in an unorganized structure? The other fact to which I alluded, occurred to a friend of mine, who had a small portion of enamel splintered off from the anterior surface of an incisor, so that a very minute point of the bone was exposed. Touching this point with any sharp instrument, occasions pain of the most pungent kind, although there is not the least *general* tenderness in the tooth itself.

Caries, the most common disease of the teeth, can only be explained by admitting their vitality. Some of the observa-

tions of the late Mr. Fox on this disease are not only obscure but inconsistent, and I shall take the liberty of making a few remarks on the subject, with the hope of in some measure elucidating it.

“The proximate cause of caries,” says Mr. Fox,* “appears to be an inflammation in the bone of the tooth, which, on account of its peculiar structure, terminates in mortification.” In the next page he endeavours to explain the mode in which this *effect* of inflammation takes place, by saying, “when the membrane” lining the cavity “becomes inflamed, it separates from the bone, and the death of the tooth is the consequence. That this is the proximate cause of caries appears highly probable,” &c. Now that the inflammation in these cases would not be confined exclusively either to the membrane or to the tooth itself, is sufficiently obvious from their intimate connexion. The teeth, although organized, certainly possess less living power than any other part of the body, and therefore cannot recover from the effects of severe inflammation, and mortification or caries of some part of the crown is the consequence. But should the inflammation be sufficient totally to destroy the connexion between the membrane and the tooth, then the whole tooth loses its vitality at once, and in some cases remains in the jaw, not having the appearance of caries, though it altogether becomes considerably darkened. This view of the subject will, I think, account for the fact, that the fang is less liable to that kind of mortification which is understood by the term caries than the crown; for as the fang appears to be originally possessed of a somewhat higher degree of vitality,† it is consequently better able to resist the effects of inflammation; but when it does lose its vitality, it is from its connexion with the system being destroyed, and its source of nourishment cut off by the separation of the membrane.

On the same principle we may explain another striking circumstance; that *caries invariably commences at the external*

* Diseases of the Teeth, p. 12.

† It is to be remembered that in addition to the membrane lining the internal cavity, the fang is also externally covered by its proper periosteum, which is intimately connected with it by innumerable vessels.

part of the bone, immediately under the enamel; for it may be reasonably concluded, that the more internal part being immediately connected with the membrane which supplies it with vessels and nerves, would be more highly organized than the external, and longer resist the effects of inflammation.

It is upon the same theory that I would attempt an explanation of the certain and regular *progress* of caries. A portion of the tooth is killed in consequence of severe inflammation, and of course becomes an extraneous body. In order to effect its removal, an increased action is set up in the contiguous portion, as in caries of bones; but here is the essential difference in its effects upon these two varieties of structure. For the reason before alluded to, viz. the imperfect organization of a tooth, this increased action itself causes the death of that part in which it is excited, and in this manner one portion after another is destroyed. Thus the same process, which in more highly organized bones is the means which nature adopts to remove an extraneous portion and get rid of the disease, becomes in this instance the cause of its continued and irremediable extension.

I believe then that inflammation and mechanical injury are the *only* causes of caries: and I thus differ from those who assert, that mere contact of a carious tooth occasions disease in one which was previously healthy. It is of importance that the truth or error of this opinion should be ascertained, and I shall therefore state the reasons which have led me to reject it; for it has been so universally believed, that even men of science and observation have suffered it to pass as an established fact, without considering whether a more philosophical and correct explanation may not be afforded of those circumstances which have given rise to it.

The whole crown of the tooth, that part in which caries almost always commences, is completely enveloped in a covering of enamel, which is composed of phosphate of lime. Now as caries originates in the bony substance, it is clear, that for disease to be produced from any merely external cause, either chemical or mechanical destruction of the enamel must

first take place. But there is no agent evolved during the decomposition of a carious tooth, which can in any possible manner act upon the enamel; therefore the mere contact of a decayed tooth cannot affect a healthy one.

Still the circumstance is so common of disease occurring in two teeth situated close together, as to render it desirable to discover some less objectionable mode of accounting for it. I believe then that it arises from one of two causes. In the first place, it is reasonable to conclude, that any cause producing inflammation and consequent caries in any one tooth, would with equal probability have the same effect on the one in contact with it; and as one may be expected to have suffered more severely than the other, it would the more speedily show symptoms of decay, and thus be supposed to *produce* the *caries* which should shortly after show itself in its immediate neighbour.

The other cause to which I referred, is the pressure of the two teeth against each other, by which the enamel of both becomes more or less mechanically broken down at the point of contact, and thus the bony substance in both is more exposed to the action of the common cause of caries.

A considerable effusion of adhesive matter around the fang now and then occurs in consequence of severe inflammation. It appears to be thrown out from the vessels of the periosteum, and sometimes produces very distressing symptoms, which may perhaps be best illustrated by the following well marked case.

A gentleman some months since requested me to remove a molar tooth of the lower jaw, which gave him the most intolerable pain, darting in paroxysms from the tooth along the lower jaw, and towards the ear. The tooth was loosened and somewhat raised in the socket, and pressure so much augmented the pain that mastication was rendered impracticable. The disease was attributed by the patient to exposure to cold; and as no appearance of disease in the substance of the tooth itself could be discovered, I should have hesitated to extract it,

were it not for the extreme severity of the pain, and my patient's earnest entreaties. The lower part of the fang was found completely enveloped in a thick coating of adhesive matter, the result of a very high degree of inflammation. In other respects the tooth appeared healthy.

In a few days, however, all the molares *of that side*, both in the upper and lower jaw, were affected in a similar manner, and to as great a degree as the one I had extracted; they were in the same way raised somewhat above their natural level in the jaw, and produced equally severe pain on pressure; the face was much swelled, the submaxillary glands enlarged, the gums highly inflamed, and the most violent pain extended along the jaws, behind the ear, and over the forehead. Instead of complying with his reiterated entreaties to remove several of the teeth, I scarified the gums freely, and ordered a brisk purgative and an abstemious plan of diet. The next day, finding that he was not relieved, I ordered some leeches to be applied to the gums to the external part of the cheek, and base of the lower jaw, a blister to be placed behind the ear, and that the bowels should be freely evacuated. Perseverance for a day or two in this plan totally removed the inflammation, and its consequences speedily disappeared; the teeth became as firm as ever, and in a short time he could masticate on that side as well as before the disease took place.

There is no doubt that in such cases as this the inflammation, if not timely checked, would terminate in suppuration. The appearances above described, may therefore be considered as constituting the first stage of those abscesses which are not unfrequently formed at the extremity of the fang, and which occasionally produce extensive disease of the alveolar processes and gums. If this be correct, a former opinion, that they arise *exclusively* from the irritation produced either by caries, or by a fang remaining as an extraneous body in the socket, must be erroneous; for, in the case above alluded to, not one of the teeth was in the least degree affected with caries.

The appearances which these abscesses assume are various. But the most remarkable specimen I have seen, is one which

was given me by my friend Mr. Wickender, of Birmingham, consisting of a double sac at the extremity of the fang the portions being distinct from each other, and each apparently connected with the canal in the fang, which is probably in this case also double.

It appears, then, that the diseases which have been mentioned, however they may differ from each other in their nature, have but one common cause, namely, INFLAMMATION; and that were early attention paid to this circumstance, and decisive means had recourse to for its removal, much pain, and much subsequent disease might be prevented. From the dense structure of the teeth, there is no possibility of their yielding in the slightest degree to the increased thickness of the membrane when inflamed; and from their being less perfectly organized than any other part, absorption goes on with extreme tardiness, even where it occurs at all. From these considerations it will appear important to prevent the occurrence of inflammation, or to remove it as speedily as possible. As a general rule, whatever produces pain in the teeth must be ultimately injurious. Taking food or liquors, the temperature of which is either much above or much below the standard heat of the body, is the most common cause of disease; and although such particulars may appear trifling, yet to those who have seen much of the effects produced by these diseases, and the anxiety and regret of those who either still suffer from them, or have been deprived of their teeth at an early period of life, the importance of any hints which may at all contribute to their preservation, will be evident.

I am aware that the opinions which I have given in the course of the foregoing observations, are, in many respects, different from those previously held on these subjects; but I have been careful not to make assertions upon any grounds short of actual observation. My aim has been to induce additional attention to a subject which appears to me to possess considerable interest, and thus to lessen, in some small degree, the extreme frequency of diseases of the teeth, which often

render persons, even scarcely beyond the prime of life, incapable of properly masticating their food, and thus not only deprive them of their comforts, but also lay the foundation of numerous diseases dependent upon imperfect digestion.

Fenchurch street, Dec. 1818.

Cases of Tumors within the Pelvis impeding Parturition; with Remarks. By SAMUEL MERRIMAN, M. D. F. L. S., Physician-Accoucheur to the Middlesex Hospital, and to the Parochial Infirmary of St. George, Hanover Square, and Consulting Physician-Accoucheur to the Westminster General Dispensary.

[From the London Medico-Chirurgical Transactions—Vol. X. Part. I.]

IN the year 1812, I communicated to this Society “a case of Difficult Parturition, occasioned by a dropsical ovary, forming a tumor in the lower part of the pelvis;” which was published in the third volume of the Transactions. Since that time some other cases of a similar nature have fallen under my observation, which I beg leave to lay before the Society, hoping that some useful practical inferences may be deduced from them.

CASE I.

Mrs. Franklin, twenty-eight years of age, was taken in labour of her first child in September, 1809, and was attended by Mr. Hanbury, a gentleman of established reputation, and very extensively engaged in midwifery practice. After the labour had lasted upwards of forty hours, Mr. H. thought it expedient to endeavour, by means of the lever, to accelerate the delivery; but being unable to gain any advantage by the use of this instrument, he requested a consultation, and Dr. Denman was called in. By his advice the perforator was employed, and the most judicious attempts were then made to extract the child, but in vain. After many hours of exertion, Dr. Denman wrote a note to me, requesting my assistance, but though I came unfatigued, and after much had been already accom-

plished, I was not able to effect the delivery in less than four hours after I began to operate. As the pelvis did not appear to be deformed, I was at a loss to account for the difficulties that had been experienced, but I presume that they arose from the tumor hereafter to be described, situated *above* the brim of the pelvis, for neither of us was sensible of the presence of such tumor within its cavity.

Mrs. Franklin, being pregnant a second time, was again attended by Mr. Hanbury, who now discovered a fleshy tumor in the vagina, very much intrenching upon the capacity of the pelvis. Fortunately for the mother the fœtus was *hydrocephalic*; this allowed the head to yield to pressure, and the child passed without any extraordinary assistance, but not till after nine hours of very severe labour.

She became pregnant a third time, and on Thursday, December 17, 1812, her labour commenced with slight pains. At 4 o'clock P. M. of the following day, the *liquor amnii* was spontaneously discharged, soon after which the pains became very strong. On Saturday the 19th, notwithstanding the violence of the throes, so little progress was made, that I was desired to visit her with my friend Mr. Hanbury. On examination I distinctly felt the tumor he had described. It occupied the back part of the pelvis, situated, however, not exactly in the middle, but rather to the left side; it pressed so firmly against the sacrum, as to impede the free passage of the finger up the rectum; and I computed that the space between its anterior surface and the symphysis pubis hardly equalled two inches in diameter. There was a good deal of elasticity in the tumor, which I believed to be formed by a diseased ovarium, and that its contents were fluid: and under this impression, and being confident that it would prove a most serious obstacle to delivery, I proposed that it should be punctured, to which Mr. Hanbury assented. But before this expedient was adopted, it was thought right to consult Dr. Denman, and both he and Mr. Croft examined the patient, but they objected to puncturing the tumor, not being satisfied that it contained a fluid; and Dr. Denman thought, from his experience of her first labour, that the superior aperture of the pelvis was so much contracted from the projection of the lumbar vertebræ,

as would render the perforator necessary, even if there were no impediment from the tumor. After much consideration, therefore, my proposition of puncturing the tumor was rejected, and it was determined to employ the perforator, if at the end of six hours no advantage was obtained from the natural pains.

At eight o'clock P. M. Mr. Hanbury made a free opening into the cranium, and evacuated some of its contents. Both he and I then tried to separate the bones, but we found the tumor so much in our way, as to frustrate all our endeavours. We therefore determined to wait several hours, hoping that the pains, which were moderately strong, and recurred regularly, might force the head lower into the pelvis. At the end of twenty hours the head was sufficiently descended to allow Mr. H. to fix a blunt hook over the lower jaw, and thus he was enabled to exert a regular and steady extracting force, by which he at length succeeded in bringing the head into the world. Having gained this advantage, he endeavoured, by the increased power thus afforded, to extract the body of the child, but so great a degree of putrefaction had by this time taken place, that the vertebræ and integuments gave way, and the head was separated from the trunk. In this situation I again saw the patient, and assisted to bring down the shoulders and complete the delivery. After various ineffectual attempts the right arm was extracted, and we hoped by means of this to draw the thorax lower, but the arm likewise gave way, and was separated from the shoulder-joint. The removal of this, however, facilitated the extraction of the left arm, and, ultimately by fixing a crotchet between the ribs, and making an opening into the distended abdomen, so as to discharge the fetid air and water which it contained, the delivery was effected, and the placenta was at the same time expelled.

The entire duration of this labour was upwards of ninety hours, and forty-one hours elapsed after the cranium was perforated, before the delivery was completed. Mrs. Franklin remained for a long time in a state of extreme danger, and her recovery was very tedious and interrupted: the unremitting care and attention of Mr. Hanbury could not prevent a

stillicidium urinæ, the consequence of her long sufferings, under which she still labours.

It has fallen to my lot to witness very many cases of delivery, after perforating the cranium, but infinitely more difficulty was experienced in this than in any other which has occurred within my knowledge. This difficulty was attributable, I am fully persuaded, to the bulk of the tumor, and not to distortion of the pelvic bones; and I still think, that to have punctured the tumor at an early period of the labour, would have been justifiable and good practice.

CASE II.

February 23, 1815, Mrs. Cobb, a patient of the Westminster General Dispensary, upward of forty years of age, in labour of her first child, was attended by Mrs. Terry, a very intelligent midwife; who, finding an extraordinary fulness occupying the hollow of the sacrum, sent to me for advice and assistance.

On examination, I found a tumor, tense, elastic, lobulated on its surface, of an oval circumscribed figure, situated between the vagina and rectum, but rather inclined to the left side of the pelvis. Conceiving that this was a displaced ovary, and that it contained a fluid, I was desirous that it should be punctured, being well convinced that there was not room for an entire child to pass; but I hoped, if the tumor could be diminished, that the child might be born alive. Under this impression, I requested my friend and colleague, Mr. Chevalier, then surgeon to that charitable institution, to see the patient, and give his opinion of the propriety of such an operation; and as he agreed with me as to the probability of success, it was decided to make the puncture.

It now became a question whether the tumor should be punctured through the vagina, as had been done in the case of Dr. John Ford's patient,* and by Mr. Park, of Liverpool,† or through the rectum; but it was thought that some advantages would accrue from puncturing through the rectum, and there the opening was made.

* Denman's Introduction to Midwifery, chap. x. sect. 7.

† Medico-Chirurgical Transactions, vol. II. p. 299.

There was no great difficulty in performing this operation, but on withdrawing the trocar we were disappointed, in not perceiving any discharge through the canula; this therefore was removed, and its extremity was then discovered to be clogged up with a substance of greyish white colour, of a granulated texture and of the consistence of honey. Though the contents of the tumor were thus ascertained to be too much inspissated to pass through the canula of a trocar, yet we hoped that when the head was forced into the lower pelvis, the pressure on the tumor would be so great as to occasion a discharge through the aperture of some of its contents, and thus diminish its size, and ultimately permit the labour to be terminated, by the natural efforts which were renewed every five minutes.

At three o'clock P. M. no material alteration was observed. The head of the child, however, had descended somewhat lower, so that the fontanelle could be felt close to the os uteri; the countenance, tongue, and pulse all gave evidence that no mischief was likely to result from longer delay, and therefore we left her till the evening.

At 10 P. M. the tumor was much altered in shape, being more diffused and softer, offering less resistance on pressure; whence we concluded that some of its contents had been evacuated, though no positive proofs of this were to be obtained. The os tincæ was more dilated, the head of the child nearly half uncovered and considerably lower: its motions had been distinctly felt by the mother in the course of the evening.—The patient, however, was much exhausted; the pains were less beneficial, and altogether the symptoms indicated the improbability of the delivery being terminated, unless by artificial assistance. Still there appeared no positive necessity for immediate recourse to the perforator, and it was therefore determined to leave her for the night under the care of her midwife, upon whose judgment I could depend to send me information, if any unpleasant symptoms should arise.

February 24, at noon, she was visited again by Mr. Chevalier, Dr. Ley, Mr. Cornwall, and myself; we found her rather more spent than last night, yet she had got some sleep, which had somewhat refreshed her. The tumor was rather more di-

minated, but the head of the child was not at all advanced. As she had passed no urine, the catheter was introduced, and about half a pint was drawn off.

As it was manifest that more delay would only add to the hazard of the case, it was now determined to use the perforator, and if possible finish the delivery. The perforation was easily effected, but the unyielding state of the os uteri prevented for the present the extraction of the head; this was, therefore, obliged to be delayed for several hours, till greater dilatability in this part should take place, and the bones of the cranium should be more easily separable. At the end of six hours Dr. Ley accompanied me to the patient's residence, when we succeeded in breaking down and extracting with our fingers, the parietal bones, and in about two hours brought the child into the world.

For some time before the head was extracted, the tumor had ceased to be felt, and after delivery it was not to be discovered; it was probable, therefore, that it had become more emptied, and had finally ascended above the brim of the pelvis, as the uterus had contracted.

The child was moderately large, the head much ossified, and the forehead turned towards the pubes.

Mrs. Cobb had a tedious and difficult recovery, if indeed it could be termed a recovery. For many weeks she was confined to her room, afterwards she was just able to crawl about by the help of a stick, but felt almost continual pain and uneasiness in the loins; she was much constipated, the abdomen became tumefied, and frequently was extremely tense. For these several complaints various modes of relief were tried with transient benefit; had her condition of life allowed of using more expensive means of cure, it is probable that more good would have been done, but she was in extreme poverty, and though many charitable persons contributed to make her situation as comfortable as possible, yet of course much was necessarily left undone.

In the summer she went into the country, where, I am informed, more decided evidences of ascites showed themselves, but by the exhibition of some powerful diuretics these symptoms were removed. After an interval of several months she

returned to town greatly emaciated, and worn down by daily exacerbations of hectic fever, which at length released her from her sufferings about eighteen months after her delivery.

On examination after death, marks of chronic inflammation of the abdomen were every where apparent. The ovarium, situated nearly in its proper position, was, together with the neighbouring parts, agglutinated into a mass of disease: when removed, the ovarium itself was about the size of a small lemon, and contained a granulated sebaceous kind of matter; there was no hair in it, nor osseous substance, except a single piece of bone, apparently a tooth, which was lying loose among the sebaceous matter, unattached to any other part; the other ovarium was healthy; the pelvis was well formed, and could have opposed no material obstacle to the birth.

This case resembles a good deal that which occurred to Dr. John Ford, as related by Dr. Denman, in which the tumor was punctured through the vagina, and the child was expelled alive by the natural efforts. "This patient recovered from her lying-in, but some time after becoming hectic, she died at the end of about six months, though from the symptoms it did not appear that the fever was occasioned either by the disease or the operation. This patient was not examined after death."*

It is, perhaps, to be regretted in Mrs. Cobb's case, when the contents of the tumor were found to be so thick as not to be discharged through the puncture, that we did not proceed to make a more free incision, but we were cautious of being too bold in performing an operation hitherto but little known; and as it did not seem impossible to deliver by lessening the head, we thought that the method pursued was, on the whole, the most judicious and proper.

* Thus is this case stated by Dr. Denman, who says he received the history from Dr. Ford himself. I have in my possession a MS. copy of the lectures of Drs. Osborne and Clarke, taken in 1789, in which a different account is given, on what authority does not appear. It says, "the tumor was punctured, and the child was born safe; the woman died of a fever which came on, and after death the rectum was found punctured. The puncture was made with scissors; had a proper instrument been used, probably that would not have happened."

CASE III.

March 21, 1816, Mrs. Barnes, midwife to the parish of St. George, Hanover Square, sent for me to Mrs. Brechford, a poor woman, who had been many hours in labour of her third child. In this case there was a tumor lying below the projection of the sacrum, but not occupying much of its concavity; it was neither so large, so much circumscribed, nor so incompressible as in the other instances that I had witnessed. The pains were acting very powerfully, and I observed, that as the head of the child was forced down, the tumor yielded and became flattened against the back of the pelvis. I determined, therefore, to try whether I could not, by my fingers, raise it above the brim of the pelvis, and introducing my hand I succeeded with very little difficulty in removing it, and thus procured room for the head to pass, which in less than an hour was protruded through the os externum. The child was alive and healthy.

This patient had a good recovery, and still continues well, not having been pregnant since. I should not have thought this tumor ovarian, but from the possibility of raising it above the brim of the pelvis, which could hardly have been accomplished, except with an encysted tumor.

CASE IV.

The following case was communicated to me by my very ingenious friend, Mr. Alfred Hardwick, of Epsom, who was called to inspect the body after death, and who very obligingly sent me the parts for a preparation.

Mrs. Shorter, having been in pain at intervals since Saturday, June 7th, 1817, became so much worse on Wednesday, the 11th, as to send for her accoucheur, who found the os uteri fully dilated, and the head resting upon the pubes; he discovered also a considerable and firm tumor pressed into the vagina, which appeared to be situated between it and the rectum, as a finger passed into each embraced a large portion of it; her pains were regular though not strong. Next day no material alteration being perceived, a consultation was requested. The gentleman consulted, considering the tumor of a compressible nature, thought that by keeping it back, should strong pains

come on, the child might probably be protruded by the natural efforts, and therefore advised to wait.

Friday, 13th. She had some sleep towards morning, her strength and spirits good, but her stomach was sick; she took diluting nourishment, and had, at her own request, a little ale. In the evening the sickness went off; the pains were less frequent through the day; her pulse was good, and she was free from restlessness.

Saturday, 14th. She had enjoyed comfortable sleeps, had taken and continued to take plenty of nourishment, and her pains were much stronger; under these circumstances sanguine hopes were entertained of a happy termination of the labour.

At half past five in the evening, upon examination, the head was found to be removed, and the part presenting to be the right shoulder. The pulse was diminished in force; it was now agreed in consultation to turn the child by bringing down the feet. The delivery of the body of the child was shortly accomplished, but the operator was foiled in repeated attempts to bring away the head, though he employed the blunt hook and crotchet; at length, however, it was accomplished by means of the last instrument. Unfortunately, most alarming symptoms took place immediately after delivery; before the placenta could be extracted, deglutition became obstructed, syncope came on, and death closed the scene in less than half an hour.

The body was opened twenty-four hours after death. The bladder was enormously distended, containing several pints of urine; and, from its flaccid state, had probably lost all contractile power previous to dissolution. The uterus had but partially contracted; it was brought forwards with a view of examining the posterior surface; the whole fundus was dark-coloured and gorged with blood. On cutting through the reflection of the peritonæum which passes from the rectum, the knife accidentally wounded the tumor, and some of its contents escaped. The tumor was situated between the cervix uteri and the rectum, forming a cushion in the hollow of the sacrum, the superior portion rising an inch or more above the projecting part of that bone; its shape was elliptical, flattened at the anterior and posterior surfaces by the pressure it had suffered; its size was that of a large orange, or the head of a fœtus at six months; it was contained in a cyst, apparently formed of the

peritoneal reflection at its superior part, and of the cellular membrane, connecting the rectum and vagina. *It was not ovarian*; the ovaria being still visible, of their proper size and in their natural situation, with regard to the uterus. From its bulk, the rectum was nearly surrounded by it, and the anterior portion of the rectum was inseparably connected to the tumor; the whole mass was soft and compressible, and although the cyst was in most parts very thin, it had not given way by the force employed in the delivery.

The contents of the tumor were regularly disposed in layers, the concave surface of one portion being exactly adapted to the convex surface of the next; and the diameter of each about the breadth of a sixpence; their colour resembled tallow, and they appeared to consist of adipocerous matter.

There was nothing else observed upon dissection that requires to be mentioned.

This woman was about forty-four years of age at the time of her death. She had been delivered of six children. In her first and sixth labours the forceps were used, but no tumor existed at the time of her former labour (the sixth) in 1812, "at least none that could be detected by the finger in a common examination."

The alteration in the position of the child, viz. its conversion from a head to a shoulder presentation, induced the accoucheurs to bring down the feet, a practice which in some similar cases has been unsuccessfully adopted. M. Baudelocque and Professor Van Doeveren, as I have already remarked,* attempted turning, but in both cases the mothers and children were lost. My friend, Mr. Howship, has favoured me with an account of another fatal case, in which the child was turned; the statement he accidentally met with in looking over the MSS. notes of the late Mr. Henry Watson, surgeon to the Westminster Infirmary, in the possession of Mr. Heaviside. "July 30, 1766, Dr. Mackenzie was mentioning a case, where, after searching a great while for the os tincae, they at last found it lying under the os pubis, being pushed forward

* Medico-Chirurgical Transactions, Vol. III.

instead of lying backwards, towards the sacrum. The belly was very large. The woman was delivered with the greatest difficulty, for after the feet had been brought down, something still prevented the head from being delivered; however, at last, after great fatigue both to the operator and patient, the child was brought away, and the woman died next day.

“ On examining the body, it was found that a large dropsical ovarium, falling into the pelvis behind the uterus, had turned the os tincæ into that unusual and unfavorable situation.”

The operation of turning seems very ill adapted to relieve a case in which the capacity of the pelvis is so much intrenched upon, as in that where an elastic tumor fills up the hollow of the sacrum; there is, however, one instance mentioned by Giffard,* in which turning succeeded, though the child did not live many minutes.

CASE V.

Thursday, October 1st, 1818, by the desire of Mr. Hanbury, I visited Mrs. Daly, thirty-five years of age, in labour of her first child. Some symptoms had occurred on Sunday, September 27th, in consequence of which she had sent for Mrs. Parsons, her midwife; but the labour not proceeding satisfactorily, though the pains increased in strength and duration, Mr. Hanbury was called in on the Tuesday.

Mr. Hanbury, finding a very large tumor in the pelvis, was aware that the labour would be difficult, but the os uteri was so little affected by the pains, that he did not think it necessary to do more than direct some aperient medicines, and afterwards an opiate. On Wednesday evening, the 30th, the os uteri became more open, and while he was engaged in making a more accurate examination, the membranes suddenly ruptured, and a large discharge of the liquor amnii took place; after which the strength of the pains increased, and he determined to remain with his patient all night, in hopes that the uterine efforts, now frequently recurring, might improve the condition of the labour; but he had the mortification of finding that no advantage was gained, which made him desirous of a consultation the next morning.

* Case 62.

On examining the patient, my fingers immediately came in contact with a large elastic tumor, very much compressing the rectum, and lying so close to the symphysis pubis, that when forcibly pushed backwards it was impossible to gain a clear space of quite an inch of conjugate diameter. The os uteri was reached with difficulty, a large portion of it was undilated, nor did it feel very dilatable. The aperture of the uterus had the peculiarity of being longitudinal, for there was no space in the pelvis to allow of its assuming a circular form; the pains were frequent and severe, and during the pains the lips of the os uteri were pressed together in the direction of the back and front of the pelvis, while their longitudinal extension was considerably increased towards the ilia. Had the os uteri taken on the usual circular dilatation, it would have been about equal to the size of a half-crown.

That this tumor was ovarian, and that it contained a fluid, would not admit of a doubt; it was therefore proposed that it should be punctured, and this being consented to, Mr. Chevalier was applied to for this purpose. Passing a small sized curved trocar up the rectum, he thrust it into the tumor, and gave discharge to about six ounces of a pale yellow fluid, of the consistence of salad oil, which was received into a basin, and a considerable quantity afterwards escaped, which could not conveniently be collected.

The tumor was by this discharge so much diminished in size, that we hoped the pains, which continued strong, might be sufficient to force the head through the pelvis, or at least that they would bring it within the grasp of the forceps; and our hopes were heightened in the course of the evening, by finding not only that the os uteri had assumed a circular form, and was much more dilated and softer, but that the head had descended somewhat through the superior aperture of the pelvis. The general state of the patient was likewise much improved, her tongue cleaner and moist, her skin temperate, her spirits calm, and her pulse open and not exceeding 90. We judged, therefore, that we were acting wisely in still leaving the case to nature.

The symptoms continued favorable till towards 8 o'clock

the following morning, by which time it became apparent that more assistance from art would be required; at ten the cranium was perforated, and in less than an hour the fœtus was extracted. The child was well sized, and had been dead ten or twelve hours.

The patient went on without an ill symptom till the next day, when she was found very feverish, with pain and soreness of the abdomen. Twenty ounces of blood were taken from her arm, and free evacuations were procured from the bowels; these were of a very offensive nature, and the relief which she experienced gave a good augury of her recovery.

Oct. 4. The symptoms continued favourable; she took a sufficiency of mild nourishment.

Oct. 5. She was found to be very languid, pulse quick and feeble, a little tendency to delirium. It was thought right to give her small quantities of wine, and rather more generous diet. In the evening the delirium increased; she sunk rapidly, and the next morning expired.

The body was opened the next day, and the following appearances were observed.

The uterus was contracted to nearly the usual size at the same period after delivery; it exhibited no marks of inflammation or disease, except a very small tubercle on its outer surface; the left fallopian tube and ovarium were deep-coloured, and a layer of coagulable lymph was lying upon them; the right ovarium was found imbedded between the vagina and the rectum; it was about the size of a sheep's bladder, and contained fatty matter, convertible by heat into the same kind of fluid as that which was received into the basin when the tumor was punctured, a large quantity of hair, and the rudiments of two or three teeth; the punctured part was looking healthy, and the ovarium itself free from inflammation.

In the viscera of the abdomen little appearance of disease existed; there was in the pericardium rather more water than usual, but the other contents of the thorax were quite healthy; the head was opened, but nothing was therein discovered to account either for the delirium or death of the patient.

On the whole, those who were present at the examina-

tion* seemed to consider the death of the patient as more attributable to exhaustion from protracted suffering, than to any organic or other mischief that could be detected by dissection.

The fluid, which on puncturing the tumor was received into a basin, after standing a short time, became congealed into a solid butyraceous mass. This occurrence was new to me, I was not aware that the fatty matter sometimes discovered in the ovaria had ever been met with in a fluid state; I was therefore induced to submit this specimen to our associate, Dr. Bostock, who has so much distinguished himself by his careful analysis of various animal fluids; and he has been so obliging as to favour me with some remarks on this subject, which will form a very valuable appendix to the case.

On a review of the cases here related and referred to,† the following remarks have presented themselves.

* Messrs. Chevalier, A. White, Pritchett, jun. Chevalier, jun. and Sweatman. Mr. Hanbury was prevented by an obstetric engagement from being present.

† A brief enumeration of all these cases is here subjoined.

Mr. Park, in the second volume of the *Medico-Chirurgical Transactions*, has related six cases, five discovered during parturition, and one in the unimpregnated state.

In the first of these the perforator was used, and the mother recovered; the tumor is supposed to have burst.

The second includes the histories of several pregnancies; in the sixth labour an incision was made through the vagina; the child was then expelled by the pains, and the mother recovered with difficulty. It is not said whether the child was born alive, but it seems probable that it was.

The third was left to nature, which expelled the child, whether alive or dead is not mentioned, but the mother died.

In the fourth case the patient was not pregnant.

In the fifth case the tumor was opened, but it was afterwards necessary to use the perforator. The mother recovered, and has had a child since.

In the sixth case the tumor was opened, the child was expelled by the pains, but was dead born; the mother recovered.

In the third volume of the *Transactions*, the writer of this gave the case of Mrs. Ellison; she was in labour of twins, the perforator was used for one child, the other was born dead, and the mother also died.

The case of a woman at the Westminster Lying-in Hospital is also mentioned; the perforator was used, and the mother died.

Dr. Denman gives two cases, from the relation of Dr. John Ford; in the first case the perforator was used, but the mother did not survive. In the second case the tumor was opened, the child was born alive, and the mother lived six months.

M. Baudelocque turned the child, it was still born, and the mother died.

Van Doeveren likewise turned, and lost both mother and child.

In Dr. Mackenzie's case the child was turned; and both mother and child died.

1. It seems evident that tumors within the pelvis obstructing delivery are of no very common occurrence, and that when they have been detected, much doubt has been entertained respecting their nature, and the proper method of treating them; it is then of importance that the attention of practitioners of midwifery should be directed towards this subject, in order that they may be guided by rational principles in the future management of such incidents.

2. On some occasions the medical attendants have not thought it necessary to take any immediate steps towards advancing the delivery, because the patient was not considered in immediate danger, and it was hoped that the tumor would prove sufficiently compressible to allow the child to pass when strong pains should come on. I would very unwillingly be thought the advocate of precipitation in the practice of midwifery, that rock on which the earlier accoucheurs were continually splitting; but I leave it to the judgment of my readers to determine, whether, in several of the instances enumerated, procrastination was not carried much beyond what sound discretion warranted, and the urgency of the case required.

Both mother and child died in the case communicated by Mr. Hardwick, and here likewise the child was turned.

In Giffard's case, turning succeeded in saving the mother, as is to be inferred, but the child, though born alive, soon died.

In Mrs. Franklin's case the perforator was employed, and the child extracted with extreme difficulty, the mother has never quite recovered. In a former pregnancy the child was expelled dead by the natural pains.

In Mrs. Cobb's case, though the tumor was opened, it was necessary to lessen the child, but the mother lived eighteen months.

In Breechcroft's case, the tumor was raised above the brim of the pelvis, the child was born alive, and the mother recovered.

In Mrs. Daly's case, opening the tumor, and afterwards the child's head, did not preserve her life.

Thus in eighteen instances of tumors in the pelvis, comprehending thirty-eight lives, it is seen that, of the women

6 recovered perfectly,
3 imperfectly,
9 died.

Of the children 2 were born alive,
1 was born alive but incapable of living,
15 were dead,
2 are uncertain; probably one was alive, the other not.

So that the lives actually preserved amounted to 12

Ditto not preserved - - - 26

3. I have been informed that a very eminent teacher of midwifery used in his lectures to assert, that "the idea of treating these tumors by puncturing them was very dangerous and highly improper;" and he taught, that on all occasions of this kind, the perforator should be early employed. But it may be doubted whether this opinion is tenable, for it has been proved that the force necessary to terminate the labour after the cranium has been perforated, has been so great as sometimes to cause the death of the patient, and sometimes to render her future life comfortless and distressful. In cases, therefore, where the tumor is found to intrench greatly upon the capacity of the pelvis, the perforator *alone* cannot be trusted to; neither does experience warrant the practice of turning and delivering by the feet.

4. I know not that the Cesarean operation has ever been applied to cases of this nature, and its constant fatality in this country would be a very great objection to employing that mode of giving relief. Yet it must be acknowledged, that if the Cesarean operation had been performed upon Mrs. Daly, at the time the puncture was made into the tumor, there would have been a great probability of preserving the child, which was then vigorous and active; and the consequences to herself could not have been more calamitous than resulted from her actual labour, conducted, as we believed, with the greatest caution and judgment. Had the tumor, in this case, been incapable of diminution by puncture, no other means of effecting delivery could have been used, than the Cesarean section, and consequently under such circumstances, that operation would have been justifiable.

5. Upon the whole, the evidence we at present possess, is most in favor of opening the tumors; for of the nine women who recovered more or less perfectly, five appear to owe their safety to this operation; and of the three children born alive, or supposed to be so, two were preserved by the same means. It may perhaps be necessary to ascertain, if possible, whether any advantage is likely to accrue from making an incision into the tumor from the vagina, rather than puncture it through the rectum. The reasons by which we were influenced in choosing the latter, were chiefly these; first, because the most de-

pending part of the tumor could be reached from the rectum; and next, because we feared that an extensive laceration of the vagina might be produced by the stretching of the parts, when the child should come in contact with the incision or aperture.

The following suggestion of Mr. Chevalier upon the subject of the after management of the tumor, is too important to be withheld. In a note which I received from him, respecting Mrs. Daly's case, he says, "I submit to you how far it might be worth while to notice a circumstance which struck me as of some importance. You will recollect, that on the examination I very easily, by my finger, dislodged the tumor from its situation between the rectum and the vagina, and returned the diseased ovarium into its place, before any dissection of the parts was attempted. Would it not, in a similar case, be right to attempt this when the uterus is contracted after delivery, to prevent the tumor from fixing, by any subsequent inflammation, in that unnatural situation, where it might impede a future labour, more than it could do if loose in the abdomen, or adherent to the side?"

Observations on the Qualities of Wine.

[From Dr. Paris's Pharmacologia.]

THE term wine is more strictly, and especially, applied to express the fermented juice of the grape, although it is generally used to denote that of any sub-acid fruit. The presence of tartar is, perhaps, the circumstance by which the grape is most strongly distinguished from all the other sub-acid fruits that have been applied to the purpose of wine-making: the juice of the grape, moreover, contains within itself all the principles essential to vinification, in such a proportion and state of balance, as to enable it at once to undergo a regular and complete fermentation; whereas, the juices of other fruits require artificial additions for this purpose; and the scientific application and due adjustment of those means, constitute the art of making wines.* It has been remarked, that all those

* For an account of which, the reader is referred to a most ingenious and interesting essay, by Dr. Macculloch, entitled, *Remarks on the Art of making Wine*,

wines that contain an excess of malic acid, are of a bad quality: hence the grand defect that is necessarily inherent in the wines of this country, and which leads them to partake of the properties of cider; for, in the place of the tartaric, the malic acid always predominates in our native fruits.

The characteristic ingredient of all wines is alcohol; and the quantity of this, and the condition or state of combination in which it exists, are the circumstances that include all the interesting and disputed points of medical enquiry. Daily experience convinces us that the same quantity of alcohol, applied to the stomach under the form of natural wine, and in a state of mixture with water, will produce very different effects upon the body, and to an extent which it is difficult to comprehend: it has, for instance, been demonstrated, that port, madeira, and sherry, contain from one-fourth to one-fifth their bulk of alcohol, so that a person who takes a bottle of either of them, will thus take nearly half-a-pint of alcohol, or almost a pint of pure brandy! and, moreover, that different wines, although of the same specific gravity, and consequently containing the same absolute proportion of spirit, will be found to vary very considerably in their intoxicating powers. No wonder, then, that such results should stagger the philosopher, who is naturally unwilling to accept any tests of difference from the nervous system which elude the ordinary resources of analytical chemistry. The conclusion was therefore drawn, that alcohol must necessarily exist in wine in a different condition from that in which we know it in a separate state; or, in other words, that its elements only could exist in the vinous liquor, and that their union was determined, and consequently alcohol produced, by the action of distillation. That it was the product, and not the educt, of distillation, was an opinion which originated with Rouelle, who asserted that alcohol was not completely formed until the temperature was raised to the point of distillation. More lately, the same doctrine was revived and promulgated by Fabroni, in the memoirs of the Florentine Academy. Gay-Lussac has however silenced the

with Suggestions for the Application of its Principles to the Improvement of Domestic Wines.

clamorous partisans of this theory, by separating the alcohol by distillation at the temperature of 66° Fah.; and, by the aid of a vacuum, it has since been effected at 56°. Besides, it has been shewn that, by precipitating the colouring matter, and some of the other elements of the wine, by sub-acetate of lead, and then saturating the clear liquor with sub-carbonate of potass, the alcohol may be completely separated without any elevation of temperature; and, by this ingenious expedient, Mr. Brande has been enabled to construct a table, exhibiting the proportions of combined alcohol which exist in the several kinds of wine. No doubt, therefore, can remain upon this subject; and the fact of the difference of effect produced by the same bulk of alcohol, when presented to the stomach in different states of combination, adds another striking and instructive illustration to those already enumerated in the course of this work, of the extraordinary powers of chemical combination in modifying the activity of substances upon the living system. In the present instance the alcohol is so combined with the extractive matter of the wine, that it is probably incapable of exerting its full specific effects upon the stomach before it becomes altered in its properties, or, in other words, digested: and this view of the subject may be fairly urged in explanation of the reason, why the intoxicating effects of the same wine are so liable to vary in degree in the same individual, from the peculiar state of his digestive organs at the time of its potation. Hitherto we have only spoken of pure wine; but it is essential to state, that the stronger wines of Spain, Portugal, and Sicily, are rendered marketable in this country by the addition of brandy, and must consequently contain uncombined alcohol; the proportion of which, however, will not necessarily bear a ratio to the quantity added, because, at the period of its admixture, a renewed fermentation is produced by the scientific vintner, which will assimilate and combine a certain portion of the foreign spirit with the wine: this manipulation, in technical language, is called *fretting-in*. The free alcohol may, according to the experiments of Fabroni, be immediately separated, by saturating the vinous fluid with sub-carbonate of potass, while the combined portion will remain undisturbed. In ascertaining the fabrication and salubrity of a wine, this circumstance ought always to constitute

a leading feature in the enquiry; and the tables of Mr. Brande would have been greatly enhanced in practical value, had the relative proportions of uncombined spirit been appreciated in his experiments; since it is to this, and not to the combined alcohol, that the injurious effects of wine are to be attributed. "It is well known," observes Dr. Macculloch, "that diseases of the liver are the most common, and the most formidable, of those produced by the use of ardent spirits: it is equally certain, that no such disorders follow the intemperate use of pure wine, however long indulged in. To the concealed and unwitting consumption of spirit, therefore, as contained in the wines commonly drank in this country, are to be attributed the excessive prevalence of those hepatic affections which are, comparatively, little known to our continental neighbours." Thus much is certain, that their ordinary wines contain no alcohol, but that which is disarmed of its virulence by the prophylactic energies of combination.

The odour or *bouquet*, and flavour, which distinguish one wine from another, evidently depend upon some volatile and fugacious principle, soluble in alcohol. This, in sweet and half fermented wines, is immediately derived from the fruit, as in those from the Frontignac and Muscat grapes; but, in the more perfect wines, as in claret, hermitage, rivesaltes, and burgundy, it bears no resemblance to the natural flavour of the fruit, but is altogether the product of the vinous process; and, in some wines, it arises from the introduction of flavouring ingredients, as from almonds in Madeira wines, as well as in those of Xeres and Saint Lucar, and hence their well-known nutty flavour. Among the ancients it was formerly, and in modern Greece it is to this day, the fashion to give a resinous flavour by the introduction of turpentine into the casks.

Wines admit of being arranged into four classes:

1. *Sweet wines*; which contain the greatest proportion of extractive and saccharine matter, and generally the least ardent spirit, though this is often rather disguised than absent. As in these wines a proportion of sugar has remained unchanged during the process of vinification, they must be considered as the results of an imperfect fermentation, and are,

in fact, mixtures of wine and sugar: accordingly, whatever arrests the progress of fermentation must have a tendency to produce a sweet wine. Thus, boiling the *must*, or drying the fruit, will, by partially separating the natural leaven and dissipating the water, occasion such a result as is exemplified by the manufacture of the wines of Cyprus, the *vino cotto* of the Italians, and the *vinum coctum* of the ancients; by that of Frontignac, the rich and luscious wines of Canary, the celebrated *tokay*, *vino tinto*, &c. of Hungary, the Italian *montefiascone*, the Persian *schiras*, the *malmsey* wines of Candia, Chio, Lesbos, and Tenedos, and those of the other islands of the Archipelago. The wines of the ancients, as Chaptal observes, were so concentrated by boiling, that they rather deserve the name of extracts or syrups than that of wines; they must have been very sweet, and but little fermented. Apparently to remedy this, they were kept for a great length of time: according to Aristotle and Galen, seven years was the shortest period necessary for keeping wine before it was fit to drink; but wines of a century old were not uncommon at the tables of the luxurious citizens of ancient Rome: and Horace boasts of his drinking *falernian*, born as it were with him, or which reckoned its age from the same consuls.

2. *Sparkling, or effervescing wines*, as champagne, are indebted for their characteristic properties to the presence of carbonic acid. They rapidly intoxicate, in consequence of the alcohol, which is suspended in, or combined with, this gas, being thus applied in a sudden and very divided state to a large extent of nervous surface: for the same reason, their effects are as transitory as they are sudden.

3. *Dry and light*. These are exemplified by the more esteemed German wines, as *Hock*, *Rhenish*, *Mayne*, *Moselle*, *Necker*, and *Elsass*, and those highly flavoured wines, *Burgundy*, *Claret*, *Hermitage*, &c. They contain a very inconsiderable degree of ardent spirit, and combine with it the effect of an acid.

4. *Dry and strong*, as *madeira*, *port*, *sherry*, &c. The name *sec*, corruptly written *sack*, signifies dry; the *sec* wine prepared at Xeres* in Spain, is called, according to our orthography,

* *Ξηρος* signifies *dry*. This is a curious coincidence.

sherris, or sherry. In the manufacture of sherry, lime* is added to the grapes; a circumstance, observes Dr. Macculloch, apparently conducive to its well known dry quality, and which probably acts by neutralizing a portion of malic or tartaric acid.

By the adulteration or medication of wines, three principal objects are attempted, viz. 1. To give them strength, which is effected by adding any ardent spirit; but the wine is slowly decomposed by it. 2. To perfect or change their colour. It is very usual to change white wines, when they have grown brown or rough, into red wines, by means of sloes or other colouring matter. 3. To lessen or remove their acidity. It is well known that lead in different forms has frequently been employed for this purpose: the practice, however, is attended with the most dangerous consequences; but which Dr. Macculloch is inclined to believe has been over-rated, since the compounds which this metal forms with the tartaric and malic acids are insoluble: but against this argument the decisive results of experience may be opposed; and Fourcroy conceived that, by the addition of lead, a soluble triple salt, an aceto-tartrate of lead, was produced. The fraud may be easily detected by the test† invented by Dr. Hahnemann. The ancients, it appears, were acquainted with this property in lead; for, according to Pliny, the Greeks and Romans improved the quality of their wines by immersing a plate of lead in them.‡ Wine, as a pharmaceutical agent, is employed to extract several of the principles of vegetables, and to dissolve certain mineral bodies.

* The *sack* of Shakspeare was probably *sherry*; a conjecture which receives additional strength from the following passage:

Falstaff.—"You rogue, here's *lime* in this sack too: there is nothing but roguery to be found in villainous man: yet a coward is worse than a cup of sack with *lime* in it; a villainous coward."

† Expose equal parts of sulphur and powdered oyster-shells to a white heat for fifteen minutes, and, when cold, add an equal quantity of cream of tartar: these are to be put into a strong bottle with common water, to boil for an hour; and the solution is afterwards to be decanted into ounce phials, adding twenty drops of muriatic acid to each. This liquor will precipitate the least quantity of lead from wines in a very sensible black precipitate. As iron might be accidentally contained in the wine, the muriatic acid is added to prevent its precipitation.

‡ Lead will not only correct the acidity of wines, but remove the rancidity of oils; a property which is well known to painters, and which affords an expedient for making an inferior olive oil pass for good.

As a solvent, however, it is liable to many serious objections, as inequality of strength and uncertainty of composition: thus, sound and perfectly fermented dry wine, as sherry, is frequently unable to dissolve iron, while tartarized antimony is instantly decomposed by every other. As a menstruum to obtain an extract, it is quite inadmissible, on account of the residuum it leaves by evaporation.

Observations, with Cases, illustrative of the Salutary Powers of the Seed of the Colchicum Autumnale, or Meadow Saffron, in the Cure of Chronic Rheumatism. BY WILLIAM HENRY WILLIAMS, M. D., F. L. S., Ipswich, Fellow of the Royal College of Physicians, London.

[From the London Medical Repository, for August, 1820.]

DAILY observation evinces the fallacy of the hopes which are continually attached to the almost innumerable host of remedies, which different writers have recommended for the cure of that species of rheumatism usually termed chronic—a painful disease, attacking principally the muscles and larger points of the body, unaccompanied by fever or inflammation of the parts affected.

In all situations of life, from the mansion to the cottage, this complaint meets the medical practitioner's view, in every variety of suffering to which it is liable; but it is peculiarly prevalent among the lower classes of society, who, subject to violent exertion and exposure to the extremes of heat and cold, and often destitute of proper clothing to check its rapid progress, linger through a protracted existence, without a prospect of cure, or alleviation to their pains, and are thus frequently deprived, for many years, of the power to support their offspring by an honest and laborious occupation.

In reflecting upon this disease, we are reminded of the sufferings of our medical oracle, the immortal Boerhaave, whose comforts were abridged, and his valuable life shortened, perhaps, by the violence of this cruel disorder; whilst the very garden in which he daily solaced himself after the fatigues at-

tendant upon his profession, and in which his botanical researches were pursued with so much ardour, might have furnished the antidote to his sufferings, if the seeds of the colchicum, which possess all the virtues of the root, without its pernicious qualities, had been presented to his imagination!

Of the properties of the colchicum root in gout and acute rheumatism, much has been said and written; but I believe the salutary powers of the *seeds* of that plant are neither generally known, nor justly estimated, either in chronic rheumatism or any other disease.

In administering the vinum seminum colchici, or colchicum seed wine, to thirty-five patients, most of whom were severely afflicted with chronic rheumatism, the effects of this medicine have really astonished me, by the early removal of pain, the progressive restoration of the use of the limbs, and the return of that salutary sleep, the want of which destroys all comfort, and embitters every enjoyment of life.

A few instances will serve to illustrate the surprising powers of this medicine, as the similarity of circumstances, in regard to positive pain, more or less usually attending each case, renders a numerous detail both tedious and unnecessary.

CASE I.

John Burbridge, aged thirty, of Upper Orwell street, Ipswich, requested my advice early in the autumn of 1817, being afflicted with severe pains in his loins, shoulders, and arms.—He was free from fever; bowels regular, and appetite good.

He followed the occupation of labourer and superintendent in a very extensive malting, for which, as an athletic man, of industrious habits, he was well adapted.

Not aware of having taken cold at an early period of his disease, he attributed his sufferings to the sudden changes of heat and cold, as well as to the vapour arising from the wetted malt, to which he was necessarily exposed; and his malady had been gradually increasing for three years, augmented by continually restless nights.

I prescribed various medicines for him during twelve months, without any material benefit; among which, a course of mercury produced no relief. His spirits began to fail, and

the future presented to him only the cheerless prospect of protracted disease, and the unavoidable necessity of relinquishing a situation which afforded the chief means of support to himself and family. In this painful state it occurred to me, early in December, to advise a fluid drachm, or a teaspoonful of the *vinum seminum colchici*, in a little ginger tea, twice in the day. After six days' trial of the medicine, he called upon me with the information that his pains had totally subsided; and a gradual increase of the dose to two spoonsful for ten days longer, afforded me the satisfaction of finding him entirely free from disease. He continues in the same state to this period, following his usual occupation.

CASE II.

George Western, aged sixty-five, resident in Cox lane, Ipswich, has been the subject of severe rheumatism in his arms and feet for some years; and at the end of December, 1817, he was under the necessity of giving up his employment, being confined to his bed for some weeks, from agonizing pain, and total inability to use his limbs. At length he was enabled to walk, or rather crawl; for he was so bowed down by his complaint, that he was altogether incapable of walking upright. In this state I met him, and having known him some years since, as coachman in a gentleman's family, I enquired what had reduced him to so forlorn a situation? Upon his replying that he was "eat up with the rheumatism," which for some weeks past had "cruelly" kept him confined to his bed, and deprived him of sleep, I offered him the *vin. sem. colch.*, to which he objected, that his advanced period of life, and infirm state, gave him no hope of being better. Upon my assuring him that I considered the prospect of his relief from pains very promising, he thankfully accepted my offered service, and I prescribed for him as I had done previously for Burbridge, and with the same happy effect; for in three weeks he was entirely recovered, and able not only to walk, but to return to his daily occupation.

In February, 1819, I accidentally met George Western again, apparently a great sufferer from rheumatism. He complained that his ankles were so painful, and his heels "so

drawn up," to use his own expression, that he never should be able to walk comfortably again. I offered him the same assistance which proved so beneficial to him upon a former occasion. In one month he was perfectly free from all pain and the sense of contraction in his heels, and continues so to this period.

CASE III.

William Potter, bricklayer's labourer, aged twenty-seven, living in Albion street, in Ipswich, became incapable of following his daily occupation early in July, 1819, from severe rheumatism in his feet and ankles, which rendered walking an exertion scarcely to be borne. About the middle of August he was admitted a patient of the dispensary, and continued there eight or nine weeks with little benefit; on which I advised him the use of the *vin. sem. colch.* in the quantity of one to two tea-spoonsful in mint water, at bed-time, for three weeks, by which time he was perfectly recovered. He has since been free from that complaint, and able to follow his usual employment.

CASE IV.

Mrs. Kedgley, aged fifty-five, residing in St. Clement's street, Ipswich, has been many years subject to rheumatism, more especially in her loins and left hip. On first visiting her, January 7, 1820, I found her in complete despair as to her recovery; in bed, to which she had been confined three months, and unable to be removed from it without experiencing the greatest agony, to which she submitted every third day, for the purpose only of having her bed made. Her account to me was, that during the greater part of 1817, the severity of the complaint confined her to the house, and also during the last eighteen months; that she was scarcely ever free from pains, which about four o'clock in the afternoon always became exceedingly severe, and continued till six o'clock, when they remitted, but returned at eight or nine in the evening, or about one o'clock in the morning, extending to the left shoulder and breast, continuing for several hours in so violent a degree as to be almost insupportable. Her bowels were slightly irregu-

lar, the excretions from them not unnatural; tongue coated with a brownish fur; little thirst, but no fever; appetite indifferent; skin neither hot nor dry; the body greatly emaciated. In this state she commenced with guaiacum and other anti-rheumatic remedies, and continued them for several weeks; taking, during a considerable part of that period, from one to two grains of opium every six or eight hours, carefully abstaining from all diet of a flatulent nature. This treatment was attended with little or no benefit; and the case being rather an extraordinary one, I wished to make fair trial of the usual remedies, in as powerful a degree as the comfort or safety of the patient would admit, before I administered the *vin. sem. colch.*; of which, about the middle of March, I prescribed daily a fluid drachm, in mint water, at three o'clock in the afternoon, and at seven or eight in the evening. She had not persevered a week in this medicine before she experienced material relief: her pains were lessened, her appetite and bodily strength were increased, and she began to enjoy a little sleep. The medicine having been gradually augmented to three fluid drachms in a dose, at the end of three weeks she was exempt from the periodical returns of pain, and was nearly free from her former sufferings; and at this period, the end of May, her health in general is renovated, her natural sleep nearly restored, and she is resuming the active duties of the mother of a considerable family, with no other symptom of the disease than occasionally a trifling pain in the hip.

It should be observed, that the distressing effects upon the stomach and bowels, too often produced by the concentrated tincture or infusion of the colchicum root, the supposed basis of most of the pretended specifics for gout, never have occurred under my observation, in administering the seeds in wine;* which I attribute principally to the seeds being free from that acrid quality so evidently prevalent in the root.

Indeed, such is the uniform mildness with which the medicine acts, that it requires no particular care in the patient, nor anxiety on the part of the medical attendant; and its effects have been always so pleasant, that the only sensible operation

* A preparation of the seeds in proof spirit is objectionable, being turbid, unpalatable, and disposed to precipitation.

has arisen from its requisite action upon the bowels once or twice a day, and the comfortable support derived from it; which is so manifest to the patient, that an omission of one or two doses usually occasions a sensation of the absence of some invigorating power.

To ensure the full efficacy of the medicine, it is desirable that diet of a flatulent nature be avoided; especially fish, broths, gruel, milk, puddings, and undressed vegetables: at the same time, a material diminution in the usual quantity of beer, tea, coffee, or chocolate, will prove highly advantageous to the patient; who, in yielding to these restrictions, may still enjoy the solid comforts of the table, with the conviction that he is thus diminishing pain, and promoting his recovery.

The vinum seminum colchici which I have been accustomed to use, is prepared as follows:

℞ Sem. Colch. Autumn. siccāt. ℥ij.

Vini Hispanici (Sherry Ang. dicti), octar. j.

Digere per dies octo vel decem, subinde agitando, dein per chartam cola* et in vasi probè clauso usui serva.

The quantity administered must be proportioned, of course, to the age and strength of the patient. With adults, I have usually commenced by giving one fluid drachm in a table spoonful or more of some aromatic water, once or twice in the day, according to the violence or continuance of the disease; gradually increasing the dose to three fluid drachms, as circumstances may require, beyond which I have never found occasion to extend it. It should be taken generally two or three hours after breakfast, and repeated at bed-time; but if the disease be mild, and of short duration, it may be requisite once only in the day, when the hour of going to bed seems to have a preference.

It may not be unnecessary to remark, in this place, that the acidum aceticum, and the spiritus ammoniæ aromaticus, imbibe the powers of the seeds in the same proportion as sherry; that an abundant aqueous extract may be obtained from the seeds, but no oil; and that water distilled from them appears totally inactive.

* As the seeds contain a large quantity of mucilage, it is desirable to pass the preparation through Dutch blotting paper, before it is filtered in the usual manner.

Having tried the extract, and a tincture prepared from the seeds and the aromatic spirit of ammonia, in the proportions just mentioned, each in two cases only, I am not competent to pronounce them efficacious to the extent my most sanguine hopes anticipate; but I think I may venture to predict, that these two preparations, and also the acetum sem. colchici, will be found very powerful remedies in subduing various convulsive, spasmodic, and other disorders, to which mankind are liable, without the dire consequences too frequently produced by our more potent remedies.

With respect, more especially, to the vinum seminum colchici, of the safety and efficacy of which I have experienced such abundant proof, I cannot contemplate an extensive use of it in many painful diseases, besides chronic rheumatism, without entertaining the hope and belief, that we have at length found the happy desideratum; a powerful yet mild medicine, capable of substituting calmness, tranquillity, and balmy sleep, in the place of pain, weariness, and restless nights—a renovation of long lost limbs and comparatively robust health, in lieu of feebleness and emaciation—in a word, affording to the poor, as well as to the rich, the cherishing prospect of prolonged life; and, during its continuance, an oblivion of many of its distressing pains.

In speaking thus favourably of the colchicum seeds, and recommending so earnestly their use to the profession, I am not urging the administration of a rare and expensive medicine, nor one which is difficult of access. A bountiful Providence has diffused around us in abundance every blessing that tends to the enjoyment of life and the amelioration of its evils; and the facility with which the colchicum plant may be procured, evinces that the remedy is as widely dispersed as the disease. Medical men, therefore, in collecting and applying the seeds to the purposes for which they were evidently designed, will confer an essential benefit on mankind, and gladden the hearts of thousands.

Observations on the Properties and Medicinal Use of Elaterium.

[From Dr. Paris's Pharmacologia.]

THIS substance spontaneously subsides from the juice of the wild cucumber, in consequence, I presume, of one of those series of changes which vegetable matter is perpetually undergoing, although we are hitherto unable to express them by any known chemical law. It is therefore not an extract, either in the chemical or pharmaceutical acceptation of the term, nor an inspissated juice; nor is it a *fecula*,* as it has been termed: the Dublin College has perhaps been most correct in simply calling it Elaterium, the name given to it by Dioscorides.

It occurs, in commerce, in little thin cakes or broken pieces, bearing the impression of the muslin upon which it was dried. Its colour is greenish, its taste bitter and somewhat acrid; and, when tolerably pure, it is light, pulverulent, and inflammable.

The early history of this medicinal substance is involved in great perplexity, each author speaking of a different preparation by the same name: for instance, the elaterium of Dioscorides must have been a very different substance from that of Theophrastus; and, wherever Hippocrates mentions the term, he evidently alludes to *any* violent purgative. "Hippocrati elaterium medicamentum est quod per alvum expurgat." (*Bod. in Theophrast.*) This will in some degree reconcile the discordant testimonies of different authors with regard to the powers of elaterium: for example, Dioscorides states its dose to be from grs. ii to ℥j; in *Ætius*, Paulus, and Actuarius, it is recommended to the extent of ℥ss; in Mesne, from ℥ss to ℥j; in Bontius (*Med. Ind.*) from ℥j to ℥ss; Massarias exhibits it in doses of grs. vj; Fernelius and Senneretus, to ℥j; Herman, from grs. v. to vij; Quincy, to grs. v; and Boerhaave does not venture to give more than grs. iv; and the practitioners of the present day limit their dose from gr. $\frac{1}{2}$ to grs. ij. Dr. Clutter-

* The juices of iris-root, arum-root, and bryony-root, and those of many other plants, allow their medicinal elements to separate and subside in a similar manner, leaving the supernatant liquid perfectly inert: if we must have a generic name to express such substances, it should be termed a *feculence*, rather than a *fecula*.

buck, with a laudable intention to discover some method of procuring this article at a cheaper rate, and at the same time of discovering some process which might ensure a preparation of more uniform strength, has lately performed a series of interesting and instructive experiments;* the results of which prove, in a satisfactory manner, "that the active principle of this plant is neither lodged in the roots, leaves, flowers, or stalks, in any considerable quantity; nor is it to be found in the body of the fruit itself, or in the seeds, but in the juice around the seeds:" the substance which spontaneously subsides from this liquor, obtained without pressure, is genuine elaterium, the quantity of which contained in the fruit is extremely small; for Dr. Clutterbuck obtained only six grains from forty cucumbers. This gentleman communicated the detail of these experiments to the president of the College of Physicians, who requested me, as professor of materia medica, to report upon them. I accordingly deemed it to be my duty to enter upon a series of new experiments, which I have lately completed, with the able assistance of Mr. Farraday, in the laboratory of the Royal Institution; the results of which will shew, that, although Dr. Clutterbuck found that an eighth part of a grain of elaterium seldom failed to purge violently, yet, strange as it may appear, that not more than one grain in ten of elaterium, as it occurs in commerce, possesses any active properties, and that this decimal part is a vegetable proximate principle, not hitherto noticed, to which I shall give the name of *Elatin*. I shall subjoin the detail of my experiments in a note;† and I think it will appear, that

* *Observations on the Nature and Preparation of the Elaterium*, read at the Medical Society of London, April 24, 1819; and which were published in the *Medical Repository*, vol. xiii. No. 67.

† PROXIMATE ANALYSIS OF ELATERIUM.

Experiments. Series First.

A. Ten grains of elaterium, obtained from a respectable chemist, and having all the sensible properties which indicated it to be genuine, were digested, for twenty-four hours, with distilled water, at a temperature far below that of boiling: *four grains* only were dissolved.

B. The solution was intensely bitter; of a brownish-yellow colour; and was not in the least disturbed by alcohol, although a solution of iodine produced a blue colour: the solution therefore contained no gum, and only slight traces of starch.

their results will authorise me to express the chemical composition of elaterium in the following manner:—

I. {	F.	Water,4
	B.	Extractive,	2.6
	B. D. J.	Fecula,	2.8
	C.	Gluten,5
	K.	Woody matter,	2.5
	H.	<i>Elatin</i> ,	} 1.2
	G.	Bitter principle	
<hr/>				
10 grains.				

C. The solution, after standing twenty-four hours, yielded a pellicle of insoluble matter, which, when burnt, appeared to resemble gluten.

D. The six grains which were insoluble in water were treated, for forty-eight hours, with alcohol of the specific gravity .817, at 66° of Fahrenheit: a green solution was obtained; but, by slow evaporation, only half a grain of solid green matter was procured. The insoluble residue obstinately adhered to, and coated, the filtre like a varnish, and completely defended the mass from the action of the alcohol: it is probable that it consisted principally of fecula.

Experiments. Series Second.

E. Ten grains of elaterium, from the same sample, were treated with alcohol of the specific gravity .817, at 66° Fah. for twenty-four hours: upon being filtered, and the residuum washed with successive portions of alcohol, the elaterium was found to have lost only 1.6 of a grain. The high specific gravity of the alcohol in this experiment was important: had it been lower, different results would have been produced.

F. The alcoholic solution, obtained in the last experiment, was of a most brilliant and beautiful green colour, resembling that of the oil of cajeput, but brighter: upon slowly evaporating it, 1.2 grains of solid green matter was obtained.

G. The solid green matter of the last experiment was treated with boiling distilled water; when a minute portion was thus dissolved, and a solution of a most intensely bitter taste, and of a brownish-yellow colour, resulted.

H. The residue, insoluble in water, was inflammable, burning with smoke and an aromatic odour, not in the least bitter: it was soluble in alkalies, and was again precipitated from them unchanged in colour. It formed, with pure alcohol, a beautiful tincture, which yielded an odour of a very nauseous kind, but of very little flavour, and which gave a precipitate with water: it was soft, and of considerable specific gravity, sinking rapidly in water; circumstances which distinguish it from common resin. In very minute quantities, it purges. It appears to be the element in which all the powers of the elaterium are concentrated, and which have been denominated *Elatin*.

I. The residuum, insoluble in alcohol, weighing 8.4 grs. (*Expt. E.*) was boiled in double distilled water, when 5.9 grs. were dissolved.

J. The above solution was copiously precipitated blue by a solution of iodine, and was scarcely disturbed by the per-sulphate of iron.

K. The part insoluble, both in alcohol and water, which was left after Experiment I. amounted to 2.5 grains: it burnt like wood, and was insoluble in alkalies.

It appears that the whole of the elatin does not separate itself from its native juice by spontaneous subsidence, and that, on this account, the supernatant liquor possesses some powers as a cathartic. We cannot be surprised, therefore, that the elaterium of commerce should be a very variable and uncertain medicine; for, independent of the great temptation which its high price holds out for adulterating it, which is frequently done with starch, it necessarily follows, that, where the active principle of a compound bears so small a proportion to its bulk, it is liable to be affected by the slightest variation in the process for its preparation, and even by the temperature of the season. Where pressure is used for obtaining the juices, a greater or less quantity of the inactive parts of the cucumber will be mixed with the elatin, in proportion to the extent of such pressure; and the elaterium will, of course, be proportionally weak.* There is one curious result obtained in my experiments, which deserves notice, viz. that there is a bitter principle in the elaterium, very distinct from its extractive matter, and totally unconnected with its activity; for I diluted the solution obtained in experiment G, and swallowed it, but it produced upon me no effect, except that which I generally experience upon taking a powerful bitter,—an increased appetite. The solution B was given to a person, but no effect whatever ensued.

Dose of good elaterium, as it occurs in commerce, is about two grains; or it is better to give it only to the extent of half a grain at a time; and to repeat that dose every hour until it begins to operate. It is probably, when thus managed, the best hydragogue cathartic which we possess.

* When it has a dark-green colour, approaching to black, is compact, and very heavy, and breaks with a shining resinous fracture, we may reject it as an inferior article.

On Colchicum. By RICHARD BATTLE, Chemist,
Fore street.

[From the London Medical Repository, for July, 1820.]

THE admitted value and importance of colchicum, and the want of professional agreement as to the medicinal powers of this root, induced the writer, some time since, to direct his attention to the investigation of its nature and properties; and agreeably to the intimation which appeared in the *Repository*, No. 75, he now proceeds to report the details and the result:—

In the spring (April,) the root does not materially vary in size and general appearance from that which is ordinarily met with. It is then of a full size, but irregularly or partially indented or hollow: at this time it is found with a small attached bulb, about the size of a bean. The growth of this small bulb, *proceeds* from the latter end of April or beginning of May, according to the season, until the latter end of June, or beginning of July, at which time it attains its full growth. The parent root appears to yield as the new production advances, and when the latter attains its full size, is no longer to be found.

The *new* root is then plump, firm, and without any indentation or hollow, and does not undergo any change of appearance from this period until the latter end of August, when, in its turn, it becomes *old*; for at this time it throws out a new bulb: from that new bulb the flower proceeds, and in the course of a very few days is fully displayed.

Between this latter period and the spring, very little apparent change takes place; the root and offset are then found as first described. These changes are, of course, subject to some variation, from soil, climate, and season.

The state and condition of the root, if subjected to experiment, illustrate the process of nature in a striking and forcible manner.

A transverse section of a bulb exposed to the temperature of 170;

If procured in the *autumn*, contracts, and when dried is shrivelled;

If procured in the *spring*, the cuticle collapses, no other part of the then remaining substance being capable of enduring the heat;

If procured in the *month of July or August*, before the new bulb is projected, it remains quite solid and firm, and has a creamy appearance.

It may be inferred from these facts:

That, this root is deprived of its power, progressively, from the time of throwing out the new bulb until its final disappearance; and that, although very little change of appearance occurs during the winter months, it really undergoes a decided change during that period.

That, August, before the new bulb is thrown out, is the proper season to procure the root.

And it may also be inferred,

That the various opinions as to the medicinal properties of colchicum have proceeded from the various states in which it has been brought into use.

The root, when removed from the ground, in August, before the new bulb appears, should be *immediately* cut into transverse slices, equal in thickness to a half-crown; to be then exposed, in wicker baskets, without placing the pieces in contact, to a temperature of 170 to 180, and so to remain until dried, which will require from two to three hours; during this time it will lose in weight about two-thirds. This process should ensue without delay after the root is procured, otherwise the flowering will proceed, although out of the ground; and when the process is complete, these transverse cuts or pieces should be carefully preserved in small boxes, in a dry situation.

The root when taken from the ground at the time recommended, and cut transversely, exhibits a milky appearance on both surfaces. The exudation is not particularly pungent; it rather impresses the tongue with a cold but peculiar sensation, which remains unabated for some time. This sensation of cold is accompanied by a sense of peculiar excitement, which is conveyed to the fauces, and continues still longer than the first-mentioned sensation of cold. Under ordinary circumstances it does not inflame the skin.

In a recent case of decided gout, attacking the second joint of the great toe, twenty drops of vin. colchici* having been given, in about half an hour a slight nausea was experienced, and upon awaking from the sound sleep which ensued, the pain had entirely ceased. The dose was repeated eight or ten hours afterwards. Two months have since elapsed, without any indication of a return of the complaint.

Specimens of this preparation are placed in the Museum of Materia Medica, St. Thomas's Hospital; and will be shown to the profession by Mr. WHITFIELD.

On Hydrophobia and Scullcap. By ISAAC CLEAVER, M. D.

[From Poulson's American Daily Advertiser.]

Mr. POULSON,

THE case of Hydrophobia given in your paper of the 30th ultimo, seems to me to be a fairly marked one of Hysteria, and not in the least calculated to establish the sanative qualities of the plant mentioned. I shall continue in this opinion, until some instances are adduced of its efficacy, when not conjoined with other articles, and when not exhibited to patients of the sex and age described; where the lancet, volatiles, and opium, are the remedies most to be depended on. Notwithstanding these views, you will do me a favour by placing in your columns the following hints. As Physicians acknowledge they have no remedy for this dreadful complaint, Hydrophobia, let the Scullcap, so much extolled, have a fair trial. But, in the first place, the plant must be fully identified; and I know mistakes have frequently arisen: for out of a dozen supposed specimens shown me this spring, by persons professing to

* Two ounces of dried Colchicum, of the description above mentioned,

Two pints of Spanish White Wine,

Two ounces of Alcohol.

The colchicum slightly bruised, added to the wine, having been occasionally shaken up, this fluid was then passed through paper, and the alcohol added.

know it, not one was the plant intended—indeed, not one belonged to the same family.

The genus *Scutellaria*, I shall avoid technicals, may be easily recognized by a common observer. The flowers are gaping, somewhat in the manner of the garden sage, generally of a blue colour, and falling off, leave four seeds, without any envelope of their own, but closely invested by the green cup, which is of two lips, and closing firmly over the seeds, serves the purpose of a proper seed-vessel, and presents the appearance of a dragoon's cap, with a very low crown, flat top, declining with considerable obliquity behind, and overtopping the brim before, in the bell-crown manner. This last mark is so strikingly diagnostic that the genus can never be mistaken.

In the vicinity of our city, we find several members of this family. The true *Scutellaria Lateriflora*, may be distinguished by the *smallness* of the flowers, which are placed on very short, somewhat recurved, footstalks, arising by pairs, at nearly equal distances from erect filiform branches, each footstalk having at its base a very small leaf. These threadlike branches are the subdivisions of larger ones, which begin to separate from the main stem, *near the ground*, giving it a more bushy appearance than its congeners. It rises only to the height of about fifteen inches; all of the same family with which it may be confounded, are much taller. The leaves, which are opposite, and each upper pair situated crosswise with those below, take their rise in common with the branches and flowers; those at the root are ovate, *id est*, resembling at the base, in their contour, the large end of an egg, those on the stem, are described as bearing some resemblance to the heart as figured on playing cards, but the point is considerably more extended, and no indenture, as far as I have seen, exists at the base; on the contrary, the lateral parts are frequently a little recurrent on the footstalk. They are all toothed at the edges, and supported on footstalks of about *half their length*. The stem is quadrangular and smooth.

Scutellaria Lateriflora, is found in low, moist, rich ground, and seems to delight particularly, in newly cleared, half drained swamps. Our neighbourhood does not produce it very abundantly, but it may be found in considerable quantity,

about half a quarter of a mile from the Schuylkill, in a new meadow or field, which is watered by a stream entering that river, just above the Falls. I have seen it at Gloucester Point. As a matter of curiosity, I send you specimens of the true plant, and shall feel a pleasure in giving samples to any persons interested, who may incline to call on me.

I send you a fine specimen of the *Alisma Plantago*, which, you will perceive, does not in the least quadrate with the sketch given in the *New York Evening Post* of last year. I have long thought that the American species has been too much confounded with that of Europe. Our plant is found at this season, in nearly dry ditches, every where. I believe it is not at this time greatly admired for its curative powers, notwithstanding the eulogiums with which it has been garnished. I hope our present acquaintance may be more successful.

I am very truly yours,

ISAAC CLEAVER.

September 1.

Liquor Morphii Citratis.

[From the *London Medico-Chirurgical Review*, for June, 1820.]

FROM Dr. A. OGLEVIE PORTER, of Bristol, we have the following Account and Formula of the *Liquor Morphii Citratis*, which we have no doubt will be gratefully received by his professional brethren.

Formula. R. Opii crudi optimi uncias quatuor; acidi citrici (cryst.) uncias duas; semel in mortario lapideo contunde; dein aquæ distillatæ bullientis octarium affunde, et intime misceantur; macera per horas viginti-quatuor; per chartam bibulosam cola; signetur preparatio—" *Liquor Morphii Citratis*."

"It is desirable (says Dr. Porter) that the profession should be in legitimate possession of a preparation of opium that might supersede the temptation of having recourse to those which belong to the empiric rather than the physician. Under this impression, I am desirous to promulgate a preparation not

inferior to any yet discovered. I consider it equal in power to the Lancasterian Black Drop, as an anodyne; and equal to it, or Battley's "Sedative," in the peculiar medicinal excellencies of those preparations, in not materially exciting the circulation.

"The preparation was suggested to my mind, by having seen a saturated solution of opium in lime-juice used with great success in alleviating arthritic pain, about twenty-five years ago, in Spanish America. This recollection, with the light thrown upon the constituent principles of opium by the French chemists, led me to try the preparation in question; and it has answered to my highest expectations. The formula I think is good: it is chemical, it is elegant, and the preparation will keep. I calculate its anodyne power at three times that of tincture of opium; yet it does not produce sensible excitement, and therefore its exhibition is seldom succeeded by heat, by head-ache, or by nausea. I have ventured to call the preparation "*Liquor Morphii Citratis*," a designation at once calculated to inform the physician of its nature, and to conceal it from the patient—points of no inconsiderable importance in practice."

Bristol, March 30, 1820.

ORIGINAL REVIEW.



FOR THE ECLECTIC REPERTORY.

Doctor Irvine on Yellow Fever.

A PAMPHLET has lately appeared, having on the title page "Irvine's Treatise on Yellow Fever." Although there is no mention made of the place or of the date of this publication, it appears to have been printed at Charleston, South Carolina, during the present year.

Dr. Irvine is a very respectable practitioner, and recommends a treatment different from any that has been heretofore proposed. Concerning the origin and nature of the fever he says,

"We will attempt in the first place to establish, that the fever is of domestic origin. It becomes necessary to do this, as the late conduct of the Boards of Health in Baltimore, New York and Philadelphia, and the opinions expressed by some of the physicians in those places, have contributed in a great degree to revive, and give fresh currency to the doctrine of contagion, which assigns a foreign derivation to the disease." p. 3.

After speaking of some objections to his theory, he adds, "All the above difficulties are, however, I conceive, to be solved, or in a measure reconciled, by advertence to a single fact, which has hitherto been unaccountably overlooked; it is this; that at every period in which the yellow fever has appeared in Charleston, the summer, or *season at large*, has been *unusually sickly*; that is, the country as well as the town has been afflicted by fevers of a fatal and violent type. I am enabled, from my own experience and observation, in the course of the last twenty years, to bear testimony to this circumstance, the philosophical application of which seems at once obvious. It is easy to perceive, that if to an atmosphere, which has

already, from various causes, become so deleterious, as to occasion *general unhealthiness, be superadded*, all those effluvia which from foul streets, sewers, &c. are constantly contaminating the air of cities, a malignant disease must necessarily be produced; and accordingly under such circumstances, the yellow fever has invariably broken out, with greater or less violence, in proportion to the sickliness of the *country, or season generally*. The causes arising within the city, acting alone, that is, during healthy summers, are never sufficient to produce the fever; Charleston being always healthy, when the country around it has continued free from any remarkable mortality. The fevers of the town and country being thus always concomitant, surely indicates, beyond the possibility of dispute, that they have a common origin, or are congenerous to the climate; though indeed widely distinguished from each other in symptoms and character." p. 5, 6.

The following remarks are worthy of notice.

"I shall proceed to offer some suggestions towards preventing a recurrence of the disease among ourselves. On this subject, Philadelphia holds out to us an example, which we cannot too closely imitate. We have, indeed, already many admirable ordinances, which only require to be revived and enforced, to place us on a level with that city, on the score of cleanliness, and excellence of police. It would be expedient, also, that the legislature should aid the city council to provide for the paving of our streets and lanes, as paved ways are more easily kept clean, than those, which are all the year deep in sand or mud, in which many animal and other putrescent remains are often buried and concealed, while the exhalations from the mud, after wet weather, contribute greatly to impair the general state of the air. It would be further advisable, that the city engineer should be employed to take the general level of the town, and that the inequalities of the streets should be filled up, or levelled away, so as to afford a regular surface, from which, by means of side drains, running to the water's edge, every offensive matter might be completely carried off. The present system of subterraneous sewers, I consider as of more detriment to the health of the city, than all the other sources of infection put together. In

addition to the effluvia, arising from the mass of obstructed matter concealed within them, the necessity of frequently opening them and returning their foul contents to the surface, from which they are never removed, so that the soil of our streets becomes a compost of every thing deleterious, is a disadvantage which calls for their obliteration, or the effectual sealing up their grates, as soon as proper side drains can be constructed." p. 14, 15.

"And though it arises like the jail, hospital and camp fevers, from the effect of crowded situations, where animal emanations, or human effluvia, meeting with miasmatic principles in the air, favourable to the production of disease, return in a morbid shape and act deleteriously on the body; it is also widely distinguished from these diseases." p. 22.

"In every view then, the yellow fever is a disease strictly *sui generis*, which cannot run into any other, or any other into it; neither of these occurrences being any more possible than that the small pox should become measles, or the latter converted into syphilis or jail fever." p. 22.

Nature of Black Vomit.

"The observation made by Dr. Physick, that the black vomit, as well as the dark matters dejected from the bowels, are secretions from the inflamed blood vessels of the stomach and intestines, is in my opinion the nearest approach which has yet been made towards a developement of the true nature of the complaint. That *morbid secretion* forms the immediate source of the disease, all its various phenomena and symptoms tend, I think, most clearly to establish. This diseased process extends, in my view of the subject, to the entire mass of the smaller arteries and blood vessels, and is carried on in every part of the system to which they reach; as is plainly evidenced by the yellow depositions under the skin and in the eyes, and the effused matters found in the brain, which are unquestionably secretions from the minute ramifications of the arteries and veins at those points. The smaller blood vessels, taking on this morbid secretory action, and thus creating disorder throughout the whole animal economy, I consider in a word, as the disease itself, constituting a peculiar vascular af-

fection, to which I should apply the term *morbus vascularis*, as more appropriate than any other designation which it has yet received. The broken down and disintegrated state of the blood, which is remarked in all genuine cases of the fever, probably excites, in the first instance, and at any rate supplies and keeps up the vitiated secretions which have been described." p. 24, 25.

"The secretions entered into by the blood vessels of the stomach, by ultimately occasioning the engorgement and distention of those organs, would necessarily produce a thickening and enlargement of the coats of the stomach itself, and thus give rise to its peculiar irritability, which forms the most distressing and unmanageable symptom of the complaint. A similar process going on in the sanguineous vessels dispersed through the substance of the brain, will account for the stupor and other cephalic affections, and particularly for the dreadful delirium, and those paroxysms of violence which characterize the more fatal and malignant cases. The remarkable suspension of all the natural secretions which occurs so early in the fever, adds, I conceive, a very strong corroboration to these views, as it takes place simultaneously with those unhealthy secretions, commenced in the lesser blood vessels. The functions of the liver seem totally suspended, while the dryness of the fauces and paucity of urine, throughout the attack, indicate a like non-performing state of the kidneys and salivary glands." p. 26.

"Symptoms.

"The particular time at which persons are attacked, seems first deserving of notice. This is generally just before day-break, at the period when the collapse of sleep is probably most complete. The seizures which occur thus early, I have observed to be usually more severe, and are more apt to terminate fatally, than such as take place in the forenoon; for persons are rarely attacked at any other than these periods, in the twenty-four hours. In the first instance the patient often awakes in violent tormina, affecting the stomach and bowels, which seem to suspend, or put off the febrile action, for it is not until the decline of the pain that the fever commences. In

common, however, the symptoms are such as have often been described; namely, a sense of lassitude with pain of the head, back and limbs, accompanied by *nausea*, and uneasiness of the stomach. The fever sometimes immediately succeeds these affections, at others it is ushered in by an obscure chill, or slight rigor. The arterial action is considerable, attended by a parched state of the skin, with flushing of the face, and redness of the eyes—the last however is not an invariable symptom. The pulse is in most cases tense and strong, but in a few instances, irregular and oppressed, while the tongue continues clean and gummy, being very seldom foul or covered with a white furr. The patient always complains of great oppression of the præcordia, and is exceedingly dispirited, sighs much and is often affected with slight delirium or wanderings of intellect. These symptoms are followed by frequent retchings, and the discharge of various foul matters from the stomach, the chief of which seems to be a bilious saburra, which is probably always more or less accumulated in the first passages. In these vomitions pure bile is sometimes thrown up, but which, as has been shown, has nothing to do with the disease, being caused altogether by the violence of the efforts to vomit, which has the effect of emulging the biliary duct, as often happens in cases of common sea sickness. Flatulence is another distressing symptom, which seems to be confined wholly to the stomach; and though the patient appears to be often on the point of getting rid of it by ucteration, a regurgitation to the stomach takes place, and this wind or gastric gas thus continues to undulate, creating great distress and restlessness. The paroxysm seldom extends beyond thirty-six hours, though it may be protracted to forty in a few instances. Its subsidence, which is never marked by any critical or perspiratory discharge, is succeeded by a remarkable quiescence, and in fact collapse of the whole system. The pulse becomes equable and natural, the skin relaxed and cold, the intellect clears up, and the patient appears to be doing well. There is sometimes, however, at this period of the complaint, a stupor, which is always a symptom of great danger—another bad sign, is a morbid irritability of the stomach, manifested by a renewal of the efforts to vomit whenever pressure is made by

the hand on that organ. It is also a very unfavourable symptom, when the patient expresses an opinion that he is well, or that very little is the matter with him. The last and most fatal effects of the disease, are an increased debility and prostration of strength, a sinking of the pulse, and an exacerbation of the irritability, pain, and heat of the stomach, followed by incessant strainings to vomit, and repeated discharges of the black vomit, or stuff resembling coffee-grounds, with a dejection of similar dark matters from the bowels. The urine at this time becomes dark and fetid, the yellow suffusion with petechiæ make their appearance, and hæmorrhages from the fauces, nostrils and other external surfaces take place. Finally, the pulse intermits, the breathing becomes laborious, the extremities gradually grow cold, and life ceases. The symptoms here detailed, are not, it is to be observed, invariable either in order or occurrence. Patients often die without either the hæmorrhages, black vomit or yellowness of the skin. The last-mentioned peculiarity sometimes shows itself immediately on the decline of the fever, and may always be regarded as a certain precursor of the formation of the black vomit. In the early part of the season, the cases usually terminate on the fourth day, reckoning always the natural day of twenty-four hours. As the season advances, they extend themselves to the sixth day, when the worst symptoms appear on the fifth. Towards the end of the summer, the disease is sometimes protracted to the eighth and thirteenth days; during this long collapse, the patient generally remains perfectly quiet, though very much prostrated, except where injudicious attendants tamper with the stomach, by improper administrations, and bring on the vomiting and other afflicting symptoms."

"In connection with this part of the subject, it is proper to observe, that strangers are frequently seized in the early part of the season, with bilious remitting and intermitting fevers; and though, where they recover from these, they are less likely to have the yellow fever, they are still liable to take it, by improper exposure or excitement of the system. As persons in this climate can never have the disease but once, there can be no doubt, that where strangers experience a second attack, either in the course of the same season, or at any subsequent

period, that the disease was mistaken in the first instance and was not yellow fever. Evils arise from a misapprehension of this sort; in the first place, an erroneous practice is adopted, for the same remedies that cure a bilious remittent, are injurious to the yellow fever patient; in the next, the person who has been illusively led to believe that he has undergone the necessary seasoning to the climate, throws off all precaution, and is liable to be carried off by the genuine disease. From the description which has been given, it will be seen, as observed in the former part of this discussion, that the fever consists of but *one paroxysm*, a circumstance which has never been sufficiently adverted to. For want of attention to this fact, the mistakes above described, have but too often been made, and fevers of a bilious character, which are mostly very manageable by common means, have been pronounced to be yellow fever, and the latter disease supposed to be cured, where it never existed." p. 31.

" Medical Treatment.

" Although there will be found a strict agreement between the theory of the disease, which has been advanced, and the medical treatment, now about to be recommended, the latter was not originally deduced from or founded upon the former. In practising in the fever, it was an obvious indication, to allay or reduce as far as possible the inquiescence of the stomach, which is so afflictive to the patient, and forms so baffling a symptom to the physician. Finding, after the trial of a variety of remedies, that the *saccharum saturni*, or *sugar of lead*, was the most effectual for the purpose, the rationale of its operation led to those views of the disease, presented in the third chapter of this essay. It may be right, before entering more directly on the present subject, to give a general idea of the powers of this medicine, and to remove the mistake respecting the supposed danger of administering it internally; a notion very prevalent with the public, but known to physicians to be without foundation. I shall adduce authorities on this point, which will be found to be perfectly satisfactory and decisive. I deem it necessary to premise, that it is not my intention to hold up this remedy as a specific in the yellow fever,

but simply to show, that its exhibition has been successful in so many cases, as to render it deserving of attention, and that its known properties adapt it, in a peculiar manner, to meet the different symptoms of the complaint. It will be in my power to corroborate my own statements respecting its efficacy by the concurring testimony of physicians of eminent character in this place, who, having tried it, highly approve of its use. Among these I will take the liberty of mentioning Dr. Samuel Wilson, Sen. whose experience, extensive practice, and distinguished standing in his profession, must give weight to his attestation on such a subject: Dr. Furman also, whose superior intellect, and medical erudition, entitle his opinions to peculiar attention, having given a full trial to this medicine, is convinced that its administration in yellow fever is attended with the highest advantages. The public production of names is always a matter of delicacy; I cannot however refuse myself the satisfaction of adding to the above, that of Dr. W. Warley, whose talents, judgment and integrity, must give impressiveness to his testimony in the minds of all who know him. He informs me, that though his experience in the disease, and with the use of the remedy, has been limited, he has been struck with the admirable effects of the lead, and thinks that it will prove a valuable remedy in the fever. I should state, that it was not until the complete failure of mercury as a cure, that I resorted to the article in question. It will hereafter be shewn, more at length, that mercury is not merely ineffectual, but inert, in the worst cases, and only operates when, by an effort of nature, the disease has taken a favourable turn, and the revived sensibility of the system enables it to act, or in other words, to salivate after the danger is past. Salivation and recovery being merely coincidental, and not connected in the order of cause and effect." p. 34.

"I shall now proceed to detail the general treatment which I have pursued in the disorder:

"Notwithstanding the prejudice which prevails against the use of emetics in this disease, they may be resorted to on the first access of the fever, and before the irritability of the stomach commences, with very great advantage. In all cases, the stomach is loaded with various foul matters, which can never

be effectually dislodged by cathartic medicines, and only to be got at by the direct operation of emetics. In the commencement, therefore, of the febrile stage, an emetic will be found useful, though its repetition is never adviseable. During the progress of the complaint, saline purgatives, occasionally administered, according to the state of the bowels, will answer every further purpose of evacuation. Great caution, however, is necessary in having recourse even to these, for the disease is marked by so much debility, that every care must be taken to avoid weakening the patient, or exhausting the vital principle. Depletion therefore, as a mode of cure, cannot be too much reprobated, as every thing should be done to conserve the strength, and keep the system easy and undisturbed. The abstraction of blood can but rarely be requisite, and the practitioner has only one great and plain rule to observe, that of *keeping his hands off the patient* as much as possible, and allowing nature to do her part, only occasionally favouring and assisting her efforts. He is above all to avoid tampering with the stomach, which is so easily exasperated into incurable irritability by administrations of an exciting quality. The ulterior effects of the disease certainly show themselves most conspicuously in the stomach and bowels; but practitioners in endeavouring to relieve these, though right in their object, have been often wrong in their practice. Bark and wine, and stimulants of all kinds, do infinite harm in this disease, and are forbidden by every view of its phenomena and symptoms. Where we have, as in the second, or collapsed stage of the complaint, a sunken pulse, livor and paleness of the countenance, with uneasiness of the stomach, and every evidence of excessive prostration, connected with irritability, to tranquillize and support the patient are surely the leading indications. Blisters are beneficial, both during the febrile action, and after its decline, and may be applied in greater or less number as the nature of the case may demand. Their application over the region of the stomach seems to produce the most good, by diverting irritation to the surface, and tending to translate secretion from the internal cavities to the skin. It is on the decline of the fever, that the exhibition of the *saccharum saturni* is recommended. It may be given in doses, of

from two and a half grains every two hours, to three grains every four hours, either in pills or solution, but always uncombined. The intervals between the doses, are to be shortened or extended as the greater or less urgency of symptoms may require. Some of the profession, who have been induced to use this medicine, have complained of being disappointed in its effects; but, in these instances, it has seldom received a fair trial, not having been resorted to until every other remedy has failed, and was thus employed too late or not persevered in sufficiently. Bark does not always cure intermittents, or mercury eradicate syphilis; and thus the *saccharum saturni*, though it deserves, I think, to be considered as a valuable remedy in yellow fever, will yet, no doubt, often fail of the desired effect. After having given the fullest trial to various methods of cure, particularly to the plan of mercurialization, and the course of super-venesection, once so strenuously recommended by Dr. Rush, and the ultra-phlebotomists of his school, I have found the simple mode of treatment above laid down, to succeed better than any other; producing a favourable result in a majority of instances, and always mitigating the symptoms even where the disease terminated fatally.

“As the yellow fever is a subject which materially concerns the interests of the city and community, I have thought it right to offer these facts, the result of long experience in the disease, to the public. I by no means flatter myself, that the remedy I have been recommending, will be very readily adopted by my professional brethren. In every science, an opposition is generally manifested to whatever wears the air of novelty or innovation; and by no body of men, is this opposition of what is new more apt to be displayed than by the members of the medical faculty. I am far however from objecting to this caution, for though it often assumes the appearance of illiberality, I am convinced that it is in the end productive of good, as it prevents the success of empiricism, and gives currency only to those just and sterling ideas, which have received their stamp in the mint of experiment and time.” p. 41.

We have thus endeavoured to give an analytical review of Dr. Irvine's pamphlet; believing it most satisfactory to the

medical world, as it is to ourselves, to shew the author's opinions and practice, from his own work, rather than to write an essay on the subject, or to enter into criticism. It would be desirable to be furnished with similar productions from all our southern cities; as it is by comparing the views of different practitioners, that we may hope to attain to just principles and sound practice. A summary view of the pamphlet may not be unimportant.

Dr. Irvine does not appear to believe that the disease is imported or contagious.

He thinks it occurs only near the sea or on tide waters, and that it never exists at Charleston, unless there is an unusual degree of sickness in the country.

That it is essentially different from the bilious remittents of our country, being a disease strictly *sui generis*; that it arises from the effect of crowded situations, and the mixture of human effluvia with miasmatic principles in the air.

That the black vomit is a secretion from the inflammatory blood vessels of the stomach and intestines.

That persons in South Carolina can never have the disease but once.

That the medical treatment consists of, first, an emetic; then saline purgatives, and blistering. On the decline of the fever he gives the acetate of lead, *saccharum saturni*, in doses of from two and an half grains every two hours to three grains every four hours, either in pills or solution, but always uncombined; the doses to be shortened or extended according to the urgency of the symptoms. Stimulants to be carefully avoided. Bleeding and mercury he disapproves of, particularly the latter, which he says irritates the stomach, and does not salivate unless the disease is yielding at the time.

SELECTED REVIEWS.

Cases of a serious Affection, chiefly occurring after Delivery, Miscarriage, &c. from various Causes of Irritation and Exhaustion; and of a similar Affection unconnected with the Puerperal State. By MARSHALL HALL, M. D., F. R. S. E., &c. &c. 8vo. pp. 96.

[From the London Medical and Physical Journal, for July, 1820.]

THIS work is another specimen of the results of the application of the author's talents for diagnostic pathology, for the purpose of illustrating a particular form of disease; and it confirms the hopes excited by his *Treatise on Diagnosis in general, and on that of some of the Disorders of the Digestive System*, that he has assumed the cultivation of semeiology as the principal object of his exertions. That he possesses eminent qualifications for such a task, is very evident; and that this part of pathology has engaged only an inferior, and inappropriate, degree of attention from the generality of modern physicians: those who have studied medicine as a science having made investigations of the causes and identical nature of diseases, without especial reference to those of analogous character, the chief object of their efforts. The ancients, on the contrary, regarded semeiological knowledge as that of the most importance to the physician, and some of the most precise and valuable indications of this kind in existence are to be found in their writings. Such information was particularly useful in the days of superstition. The prediction of the time at which a disease would terminate in health or in death, was commonly required of them; and the verification of their judgment caused them to be regarded, by the people, as beings possessing faculties which "*proxime ad deorum vim natura mortalis possint accedere*," as Cicero says of divination. It is from the knowledge they contain of this kind, that we peruse the works of HIPPOCRATES, with so much interest; and it forms the most

valuable part of the works of ARETAEUS, COELIUS, AURELIANUS, and ALEXANDER TRALLIANUS. Amongst the productions of the physicians who have lived since the revival of letters that have devoted their attention to this subject, the chief are SYDENHAM, FRIEND, DURET (*Hippoc. Coac. Prænot. Comment.*) VALESIIUS (*Aph. Hipp. Comment. septem*), PROSPER ALPINUS (*De Præsagienda Vita et Morte ægrotantium*), FIANUS (*De Signis, &c.*) LOMMIUS (*Med. Observat.*) LEROY (*Du Pronostic dans les Maladies aiguës*), PEZOLD (*De Prognosi in Acutis*), COPE (*Demonst. Med. Pract. Prognos.*), HEBERDEN, AVENBRUGGER, PINEL, and especially GRUNER, (*Semeiotice Physiol. et Pathol. generalem Complexa*), and LAENNEC.

Several authors, in treating of the diseases of the puerperal state, have cautioned practitioners against confounding those disorders dependant on what, to conceal our ignorance, we call irritation, with inflammation of the ordinary character; and many interesting observations on affections of this kind are dispersed in the works of DENMAN; in the paper by prof. FODERE, inserted a short time since in this Journal; in the writings of GUANI and RUBINI, the founders of the doctrine which immediately succeeded to Brownism in Italy, of which an exposition was given in the proëmium to the last volume of this Journal; and some very precise and judicious remarks on them, in a dissertation of FRANCK, entitled *De Venæsectionis apud Puerperas Abusu*; and in that on Puerperal Fever, by the excellent GIANINI, in the second volume of his work on Fevers in general, (*Della Natura delle Febbri, &c.* Milano, 1809.) But there is nowhere else such an accurate and lucid view of the irritative disorder alluded to as is here displayed by Dr. Hall, in his general abstract, and in his particular illustrations, of the cases which have occurred to his own observation. We agree with him in his assertion, though it is proper to state that our observations in this respect have been chiefly made in lying-in hospitals, that “the morbid affections in question constitute a great proportion among the puerperal cases, and a great majority among the fatal ones; and, of these fatal ones, many are daily rendered so by a mistaken use of the lancet;” and we estimate the work before us very highly, as it *must* lessen the

frequency of such occurrences in the practice of those who will peruse it, and who have pursued the injurious practice of which the author exposes the impropriety.

This affection appears to arise, the author says, from different sources of irritation and exhaustion, especially as concurring after the fatigue and shock which the system undergoes during labour or abortion.

“The principal source of irritation, is a disordered and loaded state of the alimentary canal; the principal source of exhaustion, uterine hæmorrhagy.

“This morbid affection is particularly apt to affect those persons who, previously to delivery, have laboured under a deranged state of the bowels, with constipation, diarrhœa, sickness, &c. It has occurred in females who had, previously to conception, been affected with that complaint which I have in a little work lately published, denominated the *Mimosis Decolor*. It has occurred in several persons, who, besides the palor and icterode complexion of that disorder, had been affected, previously to delivery, with anasarca; and it has occurred in several individuals who had suffered from aphthæ, attended by an irritable state of the stomach and bowels.

“This affection appears so be frequently induced by copious, but especially by protracted, uterine hæmorrhagy, the menorrhagia lochialis, imprudent or too copious, or long-continued, lactation, sickness, diarrhœa, &c. It frequently occurs in persons who have, previously to delivery, been reduced by venesection, and other remedies, necessary to subdue an inflammatory disease. It has been induced, or much aggravated, by misapplied venesection after confinement; and it has immediately followed the violent operation of a purge.

“This morbid affection is particularly apt to attack the delicate and feeble in constitution. It is aggravated, or even induced, by too great closeness and warmth of the patient’s room, or of the weather. The fatigue of a lingering labour, the violence of labour-pains, anxiety of mind, alarm and hurry, &c. have all appeared to be concurrent causes of this morbid affection.

“It is not impossible that improper articles of diet may also have contributed their share in causing this complaint: but

I cannot recal to my mind any fact by which this idea is substantiated. This remark must therefore be considered as conjecture only. It is also not improbable, that imprudent fatigue from too early rising, after delivery, has also had a baneful influence in inducing this malady.

“ In some cases the pain attending this affection has been experienced during the latter period of pregnancy. In these instances, is it not probable that the cause exists in the state of the bowels?

“ It may here be remarked, that some of the symptoms of the morbid affection in question, which have been continued by protracted uterine hæmorrhagy or lactation, have immediately ceased on removing the cause, by the lotion to be recommended hereafter, or by weaning. These causes of some painful affections are too apt to be overlooked in practice: head-ache, palpitation, nervousness, alarm, and sometimes delirium even, have been kept up by these unsuspected causes.”

Dissection after death has probably led to erroneous notions respecting the nature of affections of this kind, by developing appearances similar to those which result from the common form of inflammation. Whether they are identical modes of disease or not, is a question of some importance; but the most interesting considerations in regard to therapeutics are these: that the disease above designated often arises in the most debilitated state of the system, and seems to be more readily excited in proportion to the greater degree of weakness, perhaps because the body is then more susceptible of the influence of irritants, and that it is increased by the means which lessen inflammation as it ordinarily occurs in a vigorous state of constitution, and is alleviated only by stimulants; and especially by such as are qualified to restore to the vital organs their due energy, which they have lost, in some instances, only because the vitality of the system is concentrated in the uterus or intestines, in consequence of the irritation they have suffered previous to, and during, parturition. It should be considered, too, that medicines of this kind, applied to the stomach for instance, are often the most effectual means of relieving undue action in the uterus, on the principle of derivation, or

revulsion, just as a blister removes a pleurisy. They therefore act beneficially in a two-fold manner.

In some cases, the author says, this state of the system has given rise to sudden and unexpected dissolution after parturition; and, occasionally, the patient does not recover from an ill-directed bleeding. Sometimes it terminates fatally, after a more or less urgent or protracted and varied course; at others, there has been long continued indisposition. It appears principally under the following forms: "1, the acute; 2, the more continued; 3, with general symptoms; 4, with some predominant local affection; 5, as the effect, chiefly, of intestinal irritation; or, 6, of hæmorrhagy. The greater number of cases do not, however, admit of being referred to any one of these divisions distinctly or exclusively, but assume a mixed character."

All these forms are illustrated by cases, but we cannot take these into particular consideration: we must confine our analysis to a general abstract, and pass on, therefore, to the section relating to the "Description, Symptoms," &c.

When it has come on in an acute form, the first symptom has occasionally been "severe and long continued rigor succeeded by great heat of surface, great frequency of pulse, and some serious affection of the head, or of the abdomen." When the attack is slower and more insidious, "the rigor is less observed, the heat of surface perhaps absent, and there is throbbing pain of the head, with vertigo in the erect posture, or fluttering or palpitation of the heart, or oppressed, hurried, and sighing breathing, or irritability of the stomach and bowels," &c.

Great intestinal irritation is apt, the author says, to induce a sudden attack, with rigor and much febrile heat, and profuse hæmorrhagy will also produce those effects. The general course of the affection, as well as the mode of attack, is considerably influenced by the causes of it, and by the constitution of the patient. The symptoms "refer themselves in general to the head, heart, chest, stomach, bowels, uterus, the muscular system, and to different seats of pain." The author, in his preliminary and general history of this affection, considers the symptoms in each of the seats above mentioned, in succession;

and refers to the details of particular cases for the exemplification of them under the several complications in which he has observed them. We shall transcribe at length the general account above alluded to.

“ The symptoms which may be referred to this head are the following: severe pain; beating and throbbing; rushing, or cracking noises; vertigo or turning round of the room, especially on raising the head or assuming the erect position; intolerance of light, and of sound; wakefulness; starting during sleep; awaking hurried and alarmed; with faintness, palpitation, feeling of sinking, of impending dissolution, &c.; being overcome by noise, disturbance, or thinking even; and delirium.

“ The heart is, in different cases, affected with palpitation, fluttering, irregular and feeble action; there are beating and throbbing of the carotids, and sometimes even of the abdominal aorta; great rapidity, and sometimes irregularity, of the pulse; faintishness or fainting; urgent demand for the smelling bottle, fresh air, fanning, bathing of the temples; feeling of impending dissolution; incapability of bearing the erect position; and sometimes early fainting from the use of the lancet.

“ The respiration is affected in different cases with panting, hurry, sighing, great heaving, gasping, blowing; moaning, catching, &c. and, as has been stated, with urgent demand for fresh air. There is sometimes a sense of great and alarming oppression about the chest.

“ There is in some cases an irritative cough, in violent fits, or in the form of continual hecking: this cough appears to originate in the larynx or trachea.

“ The stomach is liable to become affected with irritability, sickness, retching, vomiting, hiccough, and eructation; the bowels with constipation, or diarrhœa, pain, flatus, distension, &c.

“ There are very frequently urgent restlessness, tossing about, and jactitation. In some cases various spasmodic affections have occurred.

“ The seats of pain are usually the head, the side, the iliac region, the loins, the region of the uterus, and the abdomen generally. The pain of the iliac and uterine region, and of the abdomen, is often attended with much tenderness.

“Amongst other symptoms should be mentioned the faintishness, the gasping, the feeling of dissolution, &c. of the patient; which sometimes occur after the first, and even a moderate, bleeding. In some instances the patient has expressed the utmost dread of being bled, from the feeling of aggravation of her suffering, or of dissolution, induced by it.”

When treating of the *diagnosis* of this affection, the author states that it is apt, in its various forms, to be mistaken and mistreated for inflammatory diseases of the head, chest, heart, stomach, bowels, uterus, and peritoneum; and especially for puerperal phrenitis and puerperal fever. The distinction is founded, principally, on the presence of some of the more unequivocal symptoms of the subject of this treatise, already described. In the case in which the practitioner has had recourse to blood letting, the effect of this remedy should be closely remarked: early faintness, increased frequency of the pulse, gasping, internal feeling of dissolution, unremitting pain, &c. are circumstances which ought, at least, to lead to the greatest caution and circumspection with regard to the further use of this remedy. In all cases, the author adds, the colon and rectum should be unloaded by glysters. This measure affords a source of diagnosis of the utmost importance, and in the opportunity it gives for the observation of the state of the intestinal contents.

The treatment the author inculcates consists in removing the causes, whether of irritation or exhaustion, and in obviating the effects already induced by them. Intestinal irritation, as already indicated, is considered as the most frequent cause; and the measures for the removal of this “must be at once mild and efficient; otherwise, exhaustion, on the one hand, and irritation only partially removed, on the other, may prove the source of the greatest danger. It is equally essential to give nourishment, and to avoid loading or disordering the stomach; and the benefit of the wisest plan may be counteracted, by imprudent exposure to fatigue, exertion, hurry, agitation, or anxiety.” The author next proceeds to illustrate these principles.

“Intestinal irritation must be removed by aperient medicines and by enemata.

“With regard to the former, small doses of calomel, and draughts with rhubarb and sulphat of magnesia, have appeared to me to be the best. One point is of the greatest importance,—it is the union, with the purgative, of a proper dose of opium, or of a stimulant medicine; and a point of little inferior importance, is the administration, before, during, and after, the action of the purgative medicine, of proper nourishment. With the calomel, I have given a small quantity of opium; and with the rhubarb and sulphat of magnesia, a little of the *tinctura cardamomi comp.*

“It is impossible to say too much in recommending the use of enemata. By their means the intestine is unloaded effectually, and without the exhaustion experienced from the action of efficient purgative medicine, and without disordering the stomach. It is of course needless to represent the importance of an exact inspection of the effect of enemata and of the purgative medicine administered.

“It is, in the next place, necessary to notice the means for obviating the various sources of exhaustion. If this be uterine hæmorrhagy, the following application is, I think, most effectual in arresting it: A lotion is prepared by dissolving from one to two drachms and a half of sulphat of zinc in a pint of soft water: a scroll of linen is then made of a proper form and bulk to fill the vagina; this scroll is then fully imbued with the lotion, introduced into the vagina, and renewed frequently. The same lotion may also be applied externally.

“The other remedies which I have found useful, are the *tinctura opii*, the *tinctura camphoræ comp.*, the *sp. ammoniæ aromat.*, æther, wine, and similar remedies: in one instance, opium, the *ext. hyoscyami*, and the *carbonas ammoniæ*, were combined, with the best effect. A proper combination of these remedies induces quiet sleep, prevents the exhaustion which would otherwise ensue from the administration of the purgative, and relieves many of the distressing symptoms of this alarming complaint.

“Similar objects are obtained by the due administration of nourishment: this should consist of chicken-broth, one part of milk, and two or three of water, thickened with arrow-root,

&c. Something of this kind should be given in small quantities, every hour, or oftener, and especially as stated."

Severe local pain is to be alleviated by various auxiliaries. When the head is thus affected, a cold lotion may be applied to it, and warm fomentation to the feet. Sometimes leeches to the temples may be advisable; but the author considers, that, when the head is affected from exhaustion, even leeches might appear improper. Venesection, he says, must always be a very hazardous remedy, and ought, he thinks, to be proscribed in these cases altogether. The same remarks are applicable to other cases of local pain. The French practitioners often use what they call *flying blisters* as revulsives in such cases, and we think with beneficial effects, when any important organ is the seat of much pain. The flying blisters are common blistering-plasters applied only for two or three hours, generally to the inside of the thighs or to the calves of the legs: a little redness of the skin is thus produced, without vesication, which might itself be a serious cause of exhaustion to a person already much debilitated.

In the attack of jactitation, Dr. Hall recommends a draught of laudanum and aromatic spirit of ammonia, free ventilation, &c. This part of the work concludes with some remarks on the importance of quietude and of good domestic management in cases where the occurrence of the affection under consideration may be feared, or has already taken place. One remark, in particular, amongst those last alluded to, strikes us in regard to its importance.

"In those cases in which the sleep is disturbed, and the moment of awaking is attended by great alarm and agitation, the sleep should be watched: if there be any agitation from dreaming or otherwise, the patient should be gently and cautiously awakened; and soothed and calmed if there should be alarm: and the best mode of awaking has appeared to me to be by offering a little nourishment; the mind by this means being immediately collected to understand the state of things."

A state somewhat similar to that above considered may arise after abortion, or independently of the puerperal state; and in men, from various analogous causes; and requires similar modes of treatment. This view of it was more particularly con-

sidered, as arising from intestinal disorder, in the author's essay on the "Mimoses."

The cases which are related in this work form a highly valuable part of it. The descriptions of the malady are given in them in very strong traits, and mark a rare talent for clinical observation. The deleterious effects of blood-letting are forcibly shewn, and well contrasted with the treatment the author advises. The infatuation with which some of the practitioners who attended previously to Dr. Hall had used the lancet in some of them, is lamentable, and it makes us particularly anxious to press on the attention of our readers this excellent addition to clinical medical literature.

Pathological and Practical Remarks on Ulcerations of the Genital Organs, pointing out the Characters by which they may be discriminated, shewing the consecutive Diseases to which they give rise; and containing an Inquiry into the Use of Mercury in their Treatment. By JAMES EVANS, Surgeon of His Majesty's 57th Regiment. One vol. 8vo, pp. 128. London, 1819.

[From the Medico-Chirurgical Review, for June, 1820.]

IT is said, that "Nature is always the same." We very much doubt the truth of this assertion. There is nothing fixed or stationary in any part of the solar system, as far as we can judge by distant examination; and we are quite sure that the earth, and all which it inhabit, are perpetually undergoing changes and revolutions. This mutability is not more prominent in the material than in the intellectual world. Religion, morals, politics, are so continually changing, that it is hazardous to proclaim TRUTH itself as immutable; at least, what passes for truth in this world. It is not to be supposed that PHYSIC offers an exception to so general a law—far from it! Not only does the science itself revolve as a whole, but all its parts circle round their centres, like the earth in its gyrations round the sun. The sophists of Athens did not fail to note this fluxionary state of things, and the disciples of Protagoras

asserted, with more truth than on many other occasions, that no individual ever saw the *same* thing twice; and consequently, that no two individuals, in succession, could possibly see an identical object. This sophism, if it be one, appears now to be verified in medicine. Where can we find two physicians agree about what is, or is not, the small pox?—where two surgeons who can unanimously decide on what is, or is not, the great pox? Remedies must, of course, follow the fate of diseases. Accordingly, we have the vaccinists and anti-vaccinists—mercurialists and anti-mercurialists. Whether these schisms in medicine may terminate in scepticism, as they have in religion, we know not; but, in both cases, much confusion is likely to prevail, before we have—*lucem ex fumo*.

The establishment of a correct diagnosis between syphilitic and syphiloid diseases is unquestionably a leading step towards reconciling the jarring opinions and practices which have hitherto prevailed in this department of surgery; consequently, every man who endeavours to effect this desirable object, has a claim on our gratitude, and need not anticipate censure from any critic, whose censure or applause could have any influence. We shall therefore proceed in our analytical labours, without further preface.

Mr. Evans divides his work into two Books, one on those diseases which have, the other on those affections which have *not* arisen from sexual intercourse; to which is added, an Appendix that shall be noticed in due time. The books are subdivided into chapters, corresponding to the number of diseases each book contains. Mr. Evans also classes his observations under the heads “Description, Diagnosis, Causes, and Treatment;” which is, perhaps, as lucid and useful an arrangement as he could have chosen.

BOOK I. DISEASES ARISING GENERALLY, OR ENTIRELY INDEPENDENT OF SEXUAL INTERCOURSE.

Chap. 1. Phlegmon.

Description. Most commonly situated in the loose skin covering the glans; its progress exactly resembling that of phlegmon in any other part of the body. After it suppurates

and bursts, nature endeavours to get rid of the loose integuments forming the sides of the sac by *ulceration*, which has such a spreading appearance; and unhealthy surface, as to cause much alarm. The sore, however, does not increase in depth.

Diagnosis. The progress to suppuration, the flow of matter on ulceration taking place, and the immediate diminution of size, after the discharge, well distinguish phlegmon of the penis from other affections.

Treatment. In phlegmon on the *prepuce*, cold applications are ineffectual; but they are useful when the body of the penis is the seat of the disease. In the *former* case, then, fomentations, warm emollient poultices, an early exit to the matter when formed, and a removal of the whole covering of the abscess by the knife, or *potassa fusa*, should the ulcerative process be set up, constitute the best *methodus medendi*.*

Chap. 2. *Anthrax.*

It is difficult to distinguish this formidable affection of the *membrum virile*, at first, from phlegmon. But when a lancet is pushed into it, instead of a flow of matter, only a few drops of blood and serum escape, followed soon afterwards by a projection of diseased cellular membrane, of a yellow colour. Nature endeavours to free herself from this disease by ulceration, or the sloughing process. Two valuable cases of this disease are detailed by our author.

Treatment. Warm emollient poultices; attention to the state of the skin and bowels; venesection, if much febrile action be present, and early opening, to give vent to the contents of the tumour.

Chap. 3. *Tubercula.*

Mr. Evans has met with two kinds of these; one resembling

* A cotemporary critic seems to question the propriety of our author's introducing phlegmon of the penis, since the disease is common to all other parts of the body's surface. We conceive, however, that if a man undertakes to describe all the diseases of an organ, he has no right to exclude those which are common to other parts, as well as to the organ treated of. Moreover, in the case in question, phlegmon, we see, is modified by the peculiar structure of the penis and its coverings; consequently, it demands a modified treatment. It is our opinion, therefore, that Mr. Evans would *not* "have acted judiciously in discarding it from the list of diseases of the penis." *Edinburgh Journal*, January, 1820.

what Dr. Bateman terms “Moluscum,” which is a circular, and rather flattened tumour, of the same colour as the surrounding integuments, and slightly elevated. If a lancet be pushed into it, a cream-like fluid is found within. Unless they inflame, they are not troublesome, and sometimes continue for years without exciting much attention, being considered as warts. When inflammation supervenes, they sometimes shrink up and disappear, leaving the part divested of cuticle, or occasionally ulcerous, and troublesome to heal. The other affection of this kind, is a small inflamed tumour, sometimes flattened like the former, sometimes spherical. Instead of shrinking up, when inflamed, it forms a small pustule, succeeded by a scab, which is found covering a cup that has been the lower half of the sac.

Treatment. Mr. Evans would not like to interfere with the *first* form while in its indolent stage, unless urged by the patient, when he would endeavour to excite inflammation, in imitation of the manner in which nature removes the complaint. In the *second* form, after the subsidence of inflammation, Mr. E. has sometimes found it necessary to destroy the base by repeated applications of caustic, before the sores would heal.

Chap. 4. Herpes Preputialis.

Excepting when it takes place on the *inner* surface of the prepuce, it seldom requires surgical assistance, unless from friction of the clothes or improper interference. In such case, the scab is partially or entirely removed, exposing an ulcer with a yellow, or white and plain surface, in shape frequently angular, and with abrupt edges.

“It is, however, when situated upon the inner surface of the prepuce, that the patient’s fears are most strongly excited, and the practitioner too often deceived; the circumstances of its not scabbing when it occurs in that place, of the vesicles when broken, forming each a small circular ulcer with a white or yellow surface, and of their sometimes running quickly one into the other, with the tediousness of the healing process when interfered with, have, I much fear, in my own practice, very often occasioned this disease to be mistaken, and treated erroneously.” 27.

Diagnosis. Herpes preputialis may be distinguished from venerola vulgaris, hereafter to be described, by its first appearing as a *cluster of vesicles*, whereas ven. vulg. appears as a *single pustule*. The scab in the *former* is little more than a scale; in the *latter*, it is generally of considerable size and thickness. When situated, however, on the *inner* surface of the prepuce, where no scab is formed, the diagnosis is not so easy. It has not the elevated edge and surface which venerola vulgaris exhibits about the eighth day.

Cause. Derangement of the digestive organs in general.* The *treatment* is very simple, as a weak solution of acetate of lead to allay inflammation.

Chap. 5. *Psoriasis Preputialis.*

“ This disease appears in the form of deep cracks or chasms around the margin of the prepuce, which, as is the case when the same disease affects the lip, are extremely irritable, and apt to bleed whenever any attempt is made at retraction, but which, from the loose cellular texture of the prepuce are, in this case, generally much deeper: the discharge is of a glutinous nature until the morbid action ceases, when it becomes purulent, and then the healing process begins, which is often very tedious.” 31.

This disease is apt to occasion bubo; and Mr. E. has *not* met with it in persons whose foreskin did not naturally cover the glans.

The *treatment* is diluted unguentum hydrargyri nitratum.

Chap. 6. *Ulcus Erraticum.*

This ulcer begins with a small pustule, generally on the outer skin of the body of the penis, which bursts, and forms a scab; which, if allowed to remain, enlarges, with the ill-conditioned sore that is discovered on its removal. This ulcer, if of long standing, is marked by unhealthy granulations alternating with foul excavations.

“ When it occurs upon the *body of the penis*, it extends it-

* The Edinburgh Reviewer remarks, on Mr. Evans's *Etiology* of Herpes Preputialis, that *stricture* is a frequent cause of the disease.

self upwards, in a line partaking more or less of the spiral or circular form, sometimes healing below, while it continues to ulcerate above, its edge being generally somewhat raised and thickened, and oftentimes everted.

“When it occurs on the *pubis*, it sometimes forms distinct ulcerations, which spread equally, or nearly equally, on all sides: at other times, they will extend in circles, leaving in their centres insulated portions of integument, or, in some cases, healthy granulations will be thrown up from the bottom of the sore, perhaps near its centre, on which points the healing process shall commence.

“The pain attending this sore, which is frequently severe, the patient often describes as burning.” 34.

Cause. In Mr. Evans’s experience, a long-continued, irregular, or injudicious use of mercury; or a considerable previous derangement of the system.

Diagnosis. “In its early stages, [the pustular and beginning of the ulcerative] this disease may be readily confounded with *venerola vulgaris*; but, after a time, the absence of the elevated surface, with the continuance of the ulcerative process, the alternate granulations and excavations, and the unequal extension of the sore, the ulceration perhaps spreading on one side, while it shall have stopped on the other, where healing may have commenced, will suffice to distinguish them; and for no other disease herein spoken of is this ulcer likely to be mistaken.” 40.

Treatment. This is a very unmanageable sore: the same treatment that appears to agree to-day, will frequently disagree to-morrow. The following are the applications which Mr. Evans found most useful upon the whole. A lotion of from five to ten drops of nitric acid in a pint of pure water; sulph. zinci gr. ss to gr. iij, in an ounce of water; superacet. plumbi gr. ss to an ounce of water; the same proportion of argentum nitratum; ditto of the oxymur. hydrargyri; ungt. hyd. nit. ʒj; cerat. ceræ ʒiij. M. ft. unguentum. Such are the local means. If the patient be plethoric, he should be enjoined abstinence in diet, and to avoid wine, spirits, and malt liquor, and *vice versa*. The mineral acids and sarsaparilla are occasionally useful.

“ But the remedy from which there has appeared the most decided good effects, was the blue pill, given in small quantities, so as to act gently upon the bowels, but not continued so long as to touch the mouth, especially when conjoined with occasional small doses of aperient salts.”

This ends the first book, or class of genital affections—*sine concubitu*.

BOOK II. DISEASES GENERALLY OR ENTIRELY ARISING FROM SEXUAL INTERCOURSE.

Chap. 1. *Excoriatio*.

This, when recent, appears in irregular patches, with itching, increased and altered secretion, which, if the excoriation does not soon heal, becomes purulent, the abraded surfaces assuming a yellow colour. Although our author has not seen extensive ulceration arise, in the first instance, from mere excoriation, yet he can readily believe that such may be the case in the lower ranks of life, among persons of bad constitutions and uncleanly habits.

The prepuce and glans generally participate in this excoriation, and buboes are a common consequence.

Cause. The simplest is *direct* abrasion of the cuticle from mere *friction*; but such cause is so rare, that our author never met with one well-attested instance of it.

“ The next case in point of infrequency, is where the disease takes place from too often repeated and long-continued friction, of which the denudation becomes an *indirect* consequence, it being preceded by patches of minute vesicles, which, breaking, leave the parts excoriated.” 49.

A more frequent cause, our author thinks, is, “ a greatly increased secretion of the sebaceous fluid, which, in these cases, has the appearance of cream; and which, unless prevented by frequent ablution, or certain applications, will soon give rise to the disease.” Some people are peculiarly liable to this mal-secretion, and it commonly is dependent on derangement of the *primæ viæ*, resulting from intemperance. Neglect of cleanliness, however, would appear to lead to it in any constitution.

“ But among all the causes of this disease, by far the most frequent is impure coition, by which an acrid secretion being applied, irritation, vesication, and subsequent excoriation are produced.

“ The altered secretion which, in one person, will only produce those superficial ulcerations, which from their wanting certain peculiarities, are included under the head of excoriation, will in another give rise to gonorrhœa, or venerola, or both. This circumstance, with there appearing to be a regular gradation up to venerola vulgaris, has given birth to the opinion, that most cases of excoriation arising from sexual intercourse, are but spurious kinds of the former disease; and this opinion is much strengthened, by their being occasionally followed by the same consecutive diseases.

“ The most extensive excoriations I have met with, have appeared to arise from connexion, while the glans and prepuce were in a state of irritation from neglect of cleanliness.”

Diagnosis. The excoriations arising from different causes are so nearly similar in their appearance and treatment, that diagnosis is of no consequence. Even if confounded with venerola vulgaris, or ulcus induratum, in its earliest stage, the mistake will only last a few days, and is of no practical importance.

Treatment. Attention to the bowels; cleanliness of the parts; lotions of the liquor plumbi acet. dilut. in the early stage of the disease. When more advanced, and there is appearance of indolent action in the parts, the application of sulphas cupri or of the argentum nitratum, will quicken the cure.

Chap. 2. Erysipelas.

This bears so strong a resemblance to excoriation, that it is almost impossible to distinguish them, excepting by its changing its place from one side of the glans and corona to the other. A wash composed of one part of alcohol to three or four of water is useful; also the black wash, calomel and lime water.*

* Mr. Evans directs ℥ss of calomel to ℥iiss of lime water. This is a larger proportion of calomel, we believe, than is generally used in private practice. REV.

Chap. 3. Venerola Vulgaris, aut Ulcus Elevatum.

This disease is more frequent than all the other ulcerations put together. It may be seated on any part of the body to which the cause is applied; but, of course, the body of the penis, the inner or outer surface of the prepuce, and the scrotum, are parts more frequently affected. Its progress may be divided into four stages: the pustular, ulceration, elevated, and stage of depression, or cicatrization.

The *first* stage usually commences from three to seven days after connexion, and lasts from four to six days. "It begins with itching and redness, followed by a spherical pustule, surrounded with an areola." The *second* stage is ushered in by the formation of a scab, under which, the ulcerative process still going on, matter sometimes accumulates, and occasions considerable pain. The scab successively enlarging by the exit and concretion of matter, takes a triangular or circular form, varying in colour from yellow to black. If the scab be removed in this stage, a concave ulcer is seen, with a glossy brown, reddish, or dirty yellow colour. The *third* stage, about the eighth day, is marked by an elevation of the edge, and a filling or rising up of the surface of the sore, which eventually becomes seated upon a fungus, raised above the level of the surrounding parts. This elevation of edge and surface is sometimes considerable; the base and edge being usually of a dark red colour, and, until about the fourteenth day, are most frequently surrounded by an efflorescence or areola, especially if the disease be seated on the outer part of the prepuce, or body of the penis. Between the fourteenth and eighteenth day the sore usually rises to its greatest height. The surface of the sore sometimes extends beyond its base, giving the appearance as though a ligature were tied tightly about it, or, as it were, the form of a mushroom.

"When the sore has attained its greatest height, it remains stationary for an uncertain time, after which it gradually, though perhaps very slowly, declines and heals; which process forms the *fourth* stage."

If this disease be situated where there is a certain density of cuticle, there is eventually left a permanent depression, re-

sembling that succeeding variola or vaccina. The appearance of these ulcers is somewhat modified by their locality; yet,

“Wherever may be the seat of these ulcers on the inner part of the prepuce, their characters are seldom doubtful after the ninth day; when, by drawing the skin well back, and making allowance for the form of the parts, the raised edge and surface cannot escape discovery, for though these may not be plainly discernible all round, they will be so on some one side.”

The diseased action often extends far beyond the seat of the sore; thus, if leeches be applied to the groin for the reduction of bubo, the wounds made by them will frequently become so many venerolic sores, which will go through their several stages with more or less regularity.

The time required for healing these ulcerations is generally from four to six weeks. Although the disease generally runs its course mildly, yet sometimes a high degree of constitutional irritation takes place, most commonly the forerunner of an eruptive affection.

“The more regularly the local disease goes through its several stages, the less liable is the patient to secondary symptoms.”

The *diagnosis* will be best drawn from a comparison of the *symptoms* of the venerola with those of other affections.

“The difference between this disease and chancre consists—first, in not as a general rule being followed by consecutive diseases when mercury is abstained from; secondly, in not requiring the excitement of the mercurial irritation for its cure; and thirdly, in that irritation being either useless or injurious.”

Causes. This disease almost invariably proceeds from sexual intercourse, and particularly from the application of certain diseased secretions in that act. Mr. Evans relates experiments to prove, *first*, that it may arise from the application of matter from a sore of the same description; and *secondly*, states cases to shew that it arises from the application of “an altered secretion without any breach of surface, or discernible disease in the female organs.”

“It is the custom of this place, Valenciennes, to have the public women examined at stated periods, and for this purpose a French surgeon is appointed. At these examinations I have frequently been present, and have always been surprised at the small portion of disease to be found among them; at one which I attended, no less than two hundred women of the lowest description, and of course the most frequented by soldiers, were examined, and *not one* case of disease was found among them; nevertheless the military hospitals had, and continued to have, their *usual number* of venereal cases.”*

The above shews very clearly how military hospitals may have their usual quota of venereal cases, in spite of all municipal regulations, which, in fact, completely fail in the attainment of their object. Not a doubt, indeed, can remain in the practitioner's mind, respecting the production of genital ulceration from diseased female secretions; hence the fallacy, as Hunter long ago observed, of the maxim, “*quod non habet non dare potest.*” The circumstance under consideration explains how it is that when one, two, or more men cohabit with the same woman, in a short space of time, the first, and perhaps the second may escape, while those who follow may contract the disease. The diseased secretions are generated in the course of this unnatural and immoral transaction.

Mr. Evans, in the third place, relates cases to prove that gonorrhœal matter, or the matter capable of producing gonorrhœa, will *also* occasion this disease.

“Two gentlemen had connexion with a girl, the one shortly after the other; one of them contracted *venerola vulgaris*, the other gonorrhœa; the girl was examined, she had some *discharge* from the parts, but no *ulceration*.”

M. E. has seen some cases where this disease has appeared to be a *metastasis* of gonorrhœa, and of the *ulcus induratum*, coming on after the cure of these affections. He relates two cases, which he thinks are “well calculated to excite a suspicion that this disease may arise spontaneously.” He doubts, however, the correctness and soundness of such a doctrine of *equivocal generation*.

* “By venereal cases, ulcerations are alone meant.”

Treatment. It runs a course, in despite of treatment; and except to allay irritation, nothing should be done in the first or second stage. The elevation of the surface of this sore in the third and fourth stages, would seem to indicate the destruction of the elevated portion as a means of quickening the process of cicatrization. Experience has proved that by so doing we shall only put our patients to unnecessary torture. Mercury introduced by friction or otherwise, so as strongly to affect the system, generally retards the cure, and never appears beneficial. The following is an outline of the treatment recommended by our author. In the *first* or pustular stage, which is seldom seen by the surgeon, however, friction is to be guarded against, the bowels to be kept open, and if there be much inflammation, the parts are to be kept cool with liq. plumb. acet. dil.

In the *second* stage, when there is much pain, a warm poultice, continued longer or shorter, according to circumstances. In general, after the removal of the scab by the poultice, the application of ung. ceræ will prevent its re-formation and the recurrence of pain. If the scab be small, and there is no pain, it is best not to remove it.

When the sore happens to be on the *inner* surface of the prepuce, there is generally much more irritation than when situated on a non-secreting surface. In this case a piece of lint kept constantly wet with the liq. plumb. dilut. should be applied to the part. Rest and abstemiousness, to prevent inflammation, swelling, and phymosis, are necessary; paying great attention to the state of the bowels.

In the *fourth* stage, the surface of the sore is to be daily touched with the sulphat of copper, in so light a manner, as to act as a stimulant, not as an escharotic; or the diluted ung. hyd. nit. The weak ointment of verdigris, mercurial ointment, or the common blue pill spread upon lint may be used.

Throughout the whole of the first and second stages, it is in the highest degree beneficial to confine the patient to bed, by which the formation of a bubo will often be prevented, local irritation avoided, and the chance of constitutional affections greatly lessened. If symptomatic fever run high, anti-

phlogistic measures must be put in force according to the exigency of the case.

Chap. 4. Venerola Superficialis.

This is considered by our author to be a variety, or spurious form of the preceding disease. It begins with a vesicle or pustule, which breaking, forms a crust, under which the cuticle is further removed, in a circular or oval form. The surface of the sore, when exposed, is of a healthy red colour, like a blistered part, and level with the surrounding integuments. The size is usually about that of a shilling; but if neglected, ill treated, or the constitutional symptoms run unusually high, it may increase to a much greater extent. In this disease there is always considerable constitutional derangement, and inflammation and ulceration of the tonsils and pharynx, while pains resembling acute rheumatism, accompany or follow it much more frequently than in *venerola vulgaris*; in short, our author never met with an instance of this disease which was *not* followed by consecutive affections.

Cause. Our author has reason to believe that it has no other cause than impure connection. He has seen several instances where it arose from the application of gonorrhœal matter.

Treatment. The constitutional symptoms, in this disease, being often of a highly inflammatory type, the most decisive measures should be had recourse to; “for, in this, as in the preceding disease, we not only, by these means, shorten the duration of the sore, but, in all probability, lessen the severity of the consecutive disease.” The lancet, purgatives, and diaphoretics generally, with topical applications of sedatives or stimulants, according to the irritability or sluggishness of the sore, are the means to be employed.

Chap. 5. Venerola Indurata, aut Ulcus Induratum.

This Mr. Evans believes to be another variety of *venerola vulgaris*. In the early stage, this ulcer is so distinctly marked that it may readily be confounded with several other affections; but sooner or later it assumes its characteristic feature, “the base becoming of a *cartilaginous hardness*, unless the sore

should be seated on the glans." When this disease is situated at the duplication of the prepuce behind the glans, or spreads to it, there is always a disposition for the ulceration to extend between the skin and body of the penis, in which case alone it puts on an excavated appearance, it being generally a superficial sore with a plain surface.

"A high degree of constitutional derangement sometimes takes place during the existence of these sores; in such cases mortification to a greater or less extent is no uncommon occurrence. Persons whose constitutions are injured by breathing an unhealthful atmosphere, by spare and unwholesome diet, by a long residence in hot climates, or by dissipation, appear to be most liable to this change in the disease.

"I have known gangrene take place as early as twenty-four hours after the appearance of the disease, and in less than seventy-two hours after connexion."

The hardened spots which these sores occasionally leave behind them are very apt to become again ulcerated, from want of attention to cleanliness, or from irritation by coition, &c.

Cause. Though in almost every instance the disease could be traced to impure intercourse, yet our author relates two cases, shewing that the application of gonorrhœal or other morbid matter, generated by the person affected, may produce the disease.

Treatment. "Extensive inflammation, swelling, and phymosis, are not frequently attendant on this sore, except in cases where mortification takes place, or is about to do so, and even in these, the two last are not always present. When the first occurs, whether accompanied or not by the other two, we must not only make use of the local remedies to reduce it, but as gangrene under these circumstances is in this disease much to be feared, have immediate recourse to more decisive measures.

"In some cases, where there is a disposition to gangrene, a high degree of constitutional irritation takes place; there is great anxiety, heat of skin, increased quickness and fulness of the pulse, a white or furred tongue, great pain of the part, and swelling of the prepuce; when these are present, there is no

time for hesitation; every mean must be employed to lower this destructive action; in the accomplishment of which, the lancet will prove, in almost every instance, the only thing upon which we can with safety depend. The other auxiliaries, such as cathartics, diaphoretics, cold applications, and the anti-phlogistic regimen, must not, however, be neglected, and the patient during the time should keep his bed, and his room be well ventilated and kept cool." P. 110.

Sometimes, but not generally, the above strong measures will leave a degree of debility behind requiring bark and opium, especially the latter, for establishing a healthy state both of the constitution and of the part. The separation of the sloughs will be accelerated by the application of warm poultices instead of the cold wash; and, not unfrequently, stimulating applications are useful auxiliaries. A change in the colour of the sore, to that of lividity, while the discharge turns the lint black, and has a peculiar fœtid smell, may warn us of approaching gangrene.

"In these cases, the same decisive measures must be adopted for its prevention, as for arresting its progress when commenced." P. 111.

In a few cases a most tormenting itching in the surrounding parts, where the sloughing action was present, or about to take place, and even after it had been arrested, deprived the patients of rest. An alcohol wash with some laudanum was found very useful, and also the calomel and lime water.

"In cases of mortification, the residence of the patient is a subject of the utmost importance; and, if low and swampy, or badly ventilated, should be changed to a more healthy one.

"Though in general the lancet is the remedy most to be depended on, yet it must not be indiscriminately used; for whenever the pulse is below the standard of health, opposite measures must be employed."

"In ordinary cases, the topical applications made use of in my hospital have been of the simplest nature, such as the weak solution of acetate of lead, or the alcohol wash.

"Extirpation of the hardened part, as recommended by

Celsus, has been practised by my friend, staff surgeon Murray, with the best success." P. 112.

Our analysis has extended so far, that we are unable to notice the subject discussed in the appendix. We have, however, in this article, given such an analytical view of Mr. Evans's work, as will render its merits appreciable, and its contents fully known in every quarter of the globe to which this Journal travels. Mr. Evans is evidently an accurate observer, a faithful describer, an unprejudiced narrator, and a judicious practitioner. With these qualities and qualifications, we have no doubt but Mr. Evans will complete the work, which he has here commenced, in a manner that will reflect great credit on himself, and confer a lasting benefit on the profession. We shall, therefore, look with some impatience for the second part of our author's researches and observations; nor will we be slow in making them known throughout the medical world.

A Treatise on Nervous Diseases. By JOHN COOKE, M. D., F. A. S., Fellow of the Royal College of Physicians, and late Physician to the London Hospital. In two volumes. Vol. I. *On Apoplexy, including Apoplexii Hydrocephalica, or Water in the Head; with an Introductory Account of the Opinions of Ancient and Modern Physiologists, respecting the Nature and Uses of the Nervous System.* Read at the College, as the Croonian Lectures of the year 1819. 8vo. pp. 469.

[From the London Medical and Physical Journal, for June, 1820.]

THE author remarks, in the Preface to this work, that "it was the opinion of a late eminent physician, that more real service may be rendered to medicine by the illustration of what is already known on the subject, than by any attempts to promulgate new theories, or new modes of practice. Impressed with the justice of this opinion," he continues, "and the propriety of acting upon it, I have taken considerable pains in endeavouring to collect, to arrange, and to communicate, in plain clear language, a variety of useful observations from the

best authors, both ancient and modern, respecting the principal diseases of the nervous system. If the example which I have presumed to set should be followed; if persons better qualified for the task than myself, would investigate other important diseases on a similar plan, a system of medicine would be formed, which might prove eminently useful, both by lessening the labours of the student, and affording practical facilities to persons actually engaged in the duties of the profession." The present volume is the first result of the author's exertions consonant to the views above designated, and it is his intention to treat, in a second one, of palsy and epilepsy. The treatise before us is a good exemplification of the correctness of the sentiments expressed in the latter part of the above cited paragraph; but when we contemplate, in the *Anatomie Générale* of BICHAT, what has been produced in physiology, in our own days, by the genius of one man devoted to original researches, and when we consider that pathological anatomy has not yet emerged from its infancy, we are not inclined to rate as low as the author appears to do, the value of original investigation, or of attempts to reason better than men have hitherto done, on the nature of such phenomena as have already become subjects of observation. It is, however, the character of the work before us, not the propriety of those views, that should here be the object of our consideration: to that we therefore turn.

The introductory account of the opinions of ancient and modern physiologists respecting the nature and uses of the nervous system, is, of course, not a proper subject for analysis in this place. The only remark we shall make on it is, that, though it be imperfect, it contains more interesting matter than we expected to find in so small an extent of space: a satisfactory exposition of so extensive a subject, would itself necessarily occupy several volumes; and it is the pathology of the nervous system, rather than its physiology, of which the author expressly purposes to treat. The treatise on Apoplexy is, indeed, not calculated for regular analysis, and we should, perhaps, best perform our critical functions, in stating in a few words, our judgment respecting the manner in which the author has fulfilled his intentions; but as there are but few points on which he has not adduced his own observations and opinions, we shall take a rapid view of the whole, in order that

the original parts of it may be brought forward in a clear and orderly manner.

The term *apoplexia* was employed by the Greeks, and is still used, the author remarks, to denote a disease in which the patient falls to the ground, often suddenly, and lies without sense or voluntary motion: GALEN, however, whose physiological views were astonishing for their correctness, on many occasions, excepts that of *respiration*. PAULUS ÆGINETA, in his description of it, took care to state that it is without fever; as if he saw the necessity of making a distinction between effects apparently similar, but dependant on very different causes. The Greek writers, in general, seem to have considered apoplexy and palsy as diseases of the same nature, as Dr. Cooke shews by his citations. ARETÆUS says, apoplexia, paraplegia, paresis, and paralysis, are all of the same kind, and consist in a defect of sensation, or of motion, or of both. Apoplexy is a palsy of the whole system; of mind, of sense, and of motion. Galen says, when all the nerves have lost sense and motion together, the disease is called apoplexy: when this happens to a part only, whether the right or left, it is called palsy. But we find some authors speak of apoplexy of the tongue, the arm, and the leg. CELSUS also uses the terms apoplexy and paralysis synonymously. It would appear from some passages in the writings of HIPPOCRATES, that he used the word *ἀφωνία*, to designate apoplexy without complication with palsy. In the writings of modern nosologists there is a great variety of definitions. SAUVAGES denominates it a most profound sleep, with stertorous respiration; and LINNÆUS describes it in almost the same terms. CULLEN says, "apoplexy is a disease in which the whole of the external and internal senses, and the whole of the voluntary motions, are, in some degree, abolished, whilst respiration and the action of the heart continue to be performed. By its being an affection of the whole of the power of sense and voluntary motion, he says, we distinguish it from palsy; and, by its being with the continuance of respiration and the action of the heart, it is distinguished from syncope." He excludes stertorous breathing from the necessary phenomena, as well as YOUNG and PORTAL. Dr. Cooke thinks it may be thus defined: "It is a disease in which the

animal functions are suspended, while the vital and nervous functions continue; respiration being generally laborious, and frequently attended with stertor."

This definition does not, we think, clearly mark its difference from common profound sleep with sufficient clearness and precision; and it is, perhaps, not possible to find one on the phenomena presented by the body externally, precisely applicable to it, that will mark this difference; as BOERHAAVE had indeed remarked. But we are beginning to relinquish nosology for nosography; that is, a useless subject, or one only fit for scholastic exercises, for one that is really a guide to the clinical practitioner.

In a fit of apoplexy, says the author, the patient falls to the ground, and lies as if in a deep sleep, from which he cannot be roused. In the strong paroxysm, persons are said to lie entirely deprived of sensation and motion; but the power of moving is occasionally apparent, and we cannot be certain that the power of feeling in these cases is wholly abolished. Dr. Cooke continues to remark, that he has seen patients in this disease shrink on being cupped, and move their hands towards the head, as if feeling uneasiness there. In the perfect, or strong apoplexy, the respiration is generally much impeded; but, although laborious, it is often, in the beginning of the paroxysm, slow and regular: in the middle, and towards the end, when the disease terminates fatally, it becomes frequent, weak, and irregular. Galen, Boerhaave, and Portal, measure the intensity of the disease by the degree of laborious or stertorous respiration; and Dr. Cooke also says, that in all the cases of strong apoplexy which he has seen, the respiration was laborious, slow, and stertorous, in the beginning of the paroxysm, and in those which proved fatal, this symptom, as far as he can recollect, remained, even when the breathing had become weak and irregular. In the strong apoplexy, a frothy saliva, or foam, is frequently excreted from the mouth, which is sometimes blown away from the lips with considerable force; and this phenomenon has been considered by some authors to indicate a violent disease.

The pulse, especially in the beginning of the paroxysm, is generally described as being regular, slow, full, sometimes

hard; but, in a few hours, if no amendment appear, it is said to become irregular, frequent, and weak. It has struck us that the frequency of the pulse has commonly increased in a direct ratio to the slowness of the respiration. Thus, the pulse is often about seventy when the number of inspirations is fifteen in a minute; and, as this number has fallen to eight or ten, the pulse has proportionately rose to 80 or 90. The author adduces a very interesting observation from Dr. CHEYNE, respecting the continuance of the irritability of the heart after the cessation of respiration. "Sitting," he says, "with my finger over the artery of a person who died of apoplexy, I distinctly felt the pulse beat after the last expiration." Both the latter phenomena are explicable, by the consideration that the heart does not depend on the influence of the brain for its nervous energy, whilst the actions necessary for respiration derive their motive influence from that organ. The other functions of nutrition are carried on with moderate activity: the contents of the intestines and urinary bladder are, it is true, commonly retained; but this is because the voluntary muscles of the belly cannot be called into action, the aid of which is necessary for their evacuation; and, for the same reason, vomiting is not excited without great difficulty, and it cannot be at all produced in many cases; though it is evident that the stomach retains its ordinary sensibility, for the medicines commonly employed to produce it leave marks of irritation, and often of severe inflammation, in that organ after death.

Dr. Cooke says, that "the face and whole body are sometimes cold and bedewed with clammy sweat; but more frequently the temperature of the skin is higher than natural, and is accompanied with copious perspiration." These phenomena are inexplicable on any other principles than the distinctness of the nervous system regulating the functions of nutritive or organic life, from that directing those of relative or animal life. Sense and voluntary motion are here nearly or totally suspended, and the actions of the organic system proceeded with a degree of activity even above that of the ordinary standard; and the heat of the body is increased, though respiration be lessened in frequency.

Fever is mentioned in the list of symptoms of this disease

by some writers; and Sir GILBERT BLANE says, that blood drawn from persons in the apoplectic paroxysm is almost always covered with the inflammatory crust: but Dr. Cooke believes that fever seldom accompanies it. Portal, and those who mention it, appear to confound the stupor which accompanies inflammation of the brain with simple apoplexy.

The eyes are most commonly quite closed; the cornea is dull and glassy, and the pupils are dilated in the generality of cases. But Dr. Cooke has seen them greatly and permanently contracted, in some instances almost to a point; and a physician of eminence of his acquaintance has observed the same phenomena. Aretæus and Cheyne appear to be the only authors who had previously made the same remark. The teeth, the author next states, are often closely locked together.—These two last circumstances will not surprise us when we consider how intimately and directly the iris and the muscles of the lower jaw sympathize with the alimentary canal, by means of the lenticular and upper cervical ganglions; and we know that irritation of the stomach and intestines often accompanies apoplexy, or is produced in the course of the disease by the medicines given to produce vomiting or evacuation of the bowels, without considering that neither of these actions can be effected without the aid of the abdominal muscles, which cannot be obtained whilst the state of apoplexy is very profound. It is a curious thing, too, that the mucous membrane of the stomach and intestines appears here even to be more irritable than in the state of health: it would seem that the vitality of those organs is increased in consequence of the want of expenditure of it by the organs subservient to volition. The author says, he believes the secretions are seldom much altered in this disease.

The paroxysm generally lasts from eight to twelve, twenty-four, or forty-eight hours; sometimes for a still longer period. Genuine apoplexy, Dr. Cooke thinks seldom destroys life in less than one or two hours. Sudden deaths, he remarks, are very commonly ascribed to apoplectic seizure; but they probably depend on some other affection. The effects of inhalation of carbonic acid and other deleterious gases, should not, he properly considers, be confounded with apoplexy.

When it does not prove fatal, it usually terminates in paralysis. The strong apoplexy almost always ends in death.

With respect to the appearances on dissection after death from apoplexy, Dr. Cooke has advanced nothing original; and we were not a little surprised to find no mention made of the important observations of ROUCHOUX and RIOBE, of which WEPFER, BRUNNER, and MORGAGNI had a glimpse, on the formation of cyst, round the coagula of blood in the substance of the brain.* They serve to explain many important circumstances in the consequences of apoplexy, and indicate some useful practical measures, as we hinted in our review of M. LERMINIER's paper, in a recent Number of this Journal.

A multitude of authorities are adduced by the author, to shew the influence of a wet and cold constitution of the atmosphere in the production of apoplexy; a circumstance we had occasion to point out in our Reports of Diseases during the winter of 1818-1819. On discussing the point of the influence of gluttony and spirituous potations in the production of apoplexy, the author seems not disposed to attribute so much to the latter agent as Dr. Cheyne and some other authors, unless it be carried to excess; and, except this be the case, he thinks that apoplexy seldom occurs amongst the labouring poor. Our observation has led us to form a different opinion; and, when Dr. Cooke states that he does not recollect "a single instance of the occurrence of apoplexy at the London Hospital during an attendance there of above twenty years," we are disposed to consider that this has arisen, not from the poor being but rarely affected with the disease, but because patients in a fit of apoplexy are not usually removed to hospitals. Several practitioners, whose duties call them to attend parish paupers, have informed us that the disease is common amongst them; and we have often remarked in the dissecting rooms of the medical schools in different parts of Europe, the great proportion of cases in which, especially during the latter part of autumn, old subjects have apparently died from apoplexy: effusion of blood in the cranium being present, generally with ossification of the cerebral arteries. Men who have spent their

* See the Number of this Journal for January, 1819, for an account of those observations in the Historical Sketch of the Progress of Medicine.

life in hard labour are more subject to ossification of the arteries on the approach of old age, than those of the upper ranks of society; and this cause appears to nearly equal, in regard to the production of apoplexy, the great luxury in diet of persons in general of the latter class.

When treating of the exciting causes of the disease, Dr. Cooke states that, as far as he has been able to compare symptoms with appearances on dissection, the sudden apoplexy seems always more or less connected with an effusion of blood. Effusions of serum are also amongst the exciting causes; but tumors, which are slow of formation and growth, can only be admitted into the list of remote causes. They may give occasion to head-ache, vertigo, lethargy, stupidity, blindness, epilepsy, &c. and they may, by their increase, in time, produce apoplexy, but not the sudden disease. Although an effusion of serum may give rise to apoplexy by pressure, Dr. Cooke says he is persuaded that an effusion of blood is, in a very great proportion of cases, "the exciting cause of the sudden strong disease." Our experience has led us to agree with the author, as regards the "sudden strong disease," especially in old persons; but apoplexy that has ensued after a few days of vertigo, head-ache, &c. has appeared to us to present more frequently serous effusion after death than extravasated blood; and it is in cases of this kind that no effusion whatever is discovered on dissection, in some instances, as Drs. Abercrombie and Cheyne lately, and the physicians of Breslau, HOFFMANN, CASIMIR MEDICUS, and RAHN had long since observed, when the disease soon terminates fatally. These seem to be the cases which led some of the older writers to constitute a species of apoplexy which they termed *nervous*.

The author passes in review all the most important remote causes of this disease with great care and critical acumen; and he comprises in this chapter the theories of death from hanging and drowning; which we shall transcribe, as of considerable practical importance, and because many medical practitioners seem to have no determinate opinions on this subject: they will, also, give the reader an idea of the author's manner of discussing the points he takes into consideration.

"Some physiologists are of opinion, that hanging produces

death by inducing apoplexy. M. Portal says, persons who are strangled do not die of suffocation or want of respiration; they perish from apoplexy. In proof of this, he observes, that Morgagni, Lieutaud, and other great anatomists, have mentioned in such cases a large quantity of blood found in the vessels of the brain, or in the cavities of that viscus, either without water, or mixed with water. He adds, water is sometimes found limpid and unmixed with blood. The examination of the bodies of persons who had been hanged, says M. Portal, which were formerly brought to us at the Jardin des Plantes for our lectures, furnished the same results.* Dr. Dejean, professor of medicine at Caen, convinced himself, by experiments made on living animals, that the result of strangulation was apoplexy, and that it was the effect of a congestion of blood in the brain.† Mr. Brodie informs me that he found a large quantity of blood extravasated in the brain of a man who had been hanged; and that Dr. Hooper has in his possession a preparation of the brain of a person who died in a similar way which exhibits a great deal of blood effused among the membranes. Some modern physiologists, however, do not agree in sentiment with M. Portal. Dr. Curry mentions an experiment, hereafter to be related, which appears conclusive on the subject.

“ It was formerly a common opinion, that drowning kills by inducing apoplexy; but Cullen, Hunter, Goodwyn, and others, maintain, that death from drowning entirely depends upon the obstruction of respiration, which takes place under these circumstances. Dr. Cullen, in his letter to Lord Cathcart, concerning the recovery of drowned persons, ‘ considers it probable that the death which ensues, or seems to ensue, in drowned persons, is owing to the stoppage of respiration, and to the ceasing, in consequence, of the circulation of the blood, whereby the body loses its heat, and with that the activity of the vital principle.’ Mr. Hunter says, ‘ the loss of motion in drowning arises from the loss of respiration;’ and he thought that this was the first cause of the cessation of the motion of the heart. Dr. Goodwyn agrees in opinion with Dr. Cullen and

* Portal, p. 300.

† *Ibid*, p. 302.

Mr. Hunter. Dr. Goodwyn made many experiments with a view to ascertain the effects of submersion upon living animals, from which he draws the following conclusions: ‘That a small quantity of water commonly passes into the lungs in drowning; that the water enters into the lungs during the efforts to inspire, and, mixing with the pulmonary mucus, occasions the frothy appearance mentioned by authors; that the whole of this fluid in the lungs is not sufficient to produce the changes which take place in drowning: whence it follows,’ he says, ‘that the water produces all the changes which take place in drowning, indirectly, by excluding the atmospheric air from the lungs, not directly, by entering into the cavity of the lungs.’”

“Dr. Goodwyn takes great pains to investigate the cause and manner of death from drowning, and is by no means disposed to consider the disease to be apoplexy which is produced by submersion. Under these circumstances, death, he thinks, takes place because the blood cannot receive oxygen from the air by respiration, without which the heart cannot be stimulated to action. ‘When the pulmonary blood,’ says Dr. Goodwyn, ‘is no longer fitted to excite the sinus venosus and auricle to contraction, they receive it into their cavity, and remain at rest. As soon as they cease to contract and propel the blood to the head, all the intellectual operations cease, and voluntary motions are suspended, and the external signs of life disappear.’ The disease, Dr. Goodwyn thinks, is in the blood, and consists in the presence of this black blood in the left side of the heart and arterial system, and might with more propriety be named *melanæma*.* Under *melanæma*, as a genus, Dr. Goodwyn would place the disease brought on by hanging and drowning; as, *melanæma à suspensione*, *melanæma à submersione*.

“Dr. Curry thinks that, in hanging, as well in drowning, the exclusion of air from the lungs is the immediate cause of death. ‘From the great accumulation of blood in the vessels of the head,’ says Dr. Curry, ‘many have been of opinion, that hanging kills chiefly by inducing apoplexy; but the

* *Melanaima*.

following ingenious experiment, made at Edinburgh many years ago by Dr. Monro, sen. clearly proves that the exclusion of air from the lungs is the immediate cause of death. A dog was suspended by the neck with a cord, an opening having been previously made in the windpipe below the place where the cord was applied, so as to admit air into the lungs. In this state he was allowed to hang for three quarters of an hour, during which time both the circulation and breathing went on. He was then taken down, without appearing to have suffered much from the experiment. The cord was now shifted from above to below the opening made into the windpipe, so as to prevent the ingress of air into the lungs; and the animal being again suspended, he was completely dead in a few minutes.'”*

We cannot attempt to abridge even the most important parts of this chapter, it being itself a concise view of matters comprising a multitude of important considerations; and it peremptorily calls forth the expression of our sentiments, which we generally endeavour to render apparent by what we produce from an author, rather than in express terms, of the great merit and practical utility of the work. The research on which the author's history of the disease has been founded, is original and very extensive; but we cannot say quite complete; as no notice is taken of some important German authors who have written in their vernacular language. This fault runs through the whole work; and, as of late years, most of them have ceased to use the Latin, a great deal of important matter, in clinical observations, has escaped the author's attention. We must, however, expressly state, in respect to the last chapter, that Dr. Cooke has not noticed the connexion of apoplexy with hypertrophy of the heart, mentioned in the Proëmium to the present volume of this Journal; a connexion of disease that, it appears to us, possesses a remarkable degree of interest, and of which the knowledge is of considerable practical importance.

The *distinctions* of apoplexy are next considered. The author says, “Dr. Cullen's division of apoplexy into species, so far as they depend upon evident causes leading to a corres-

* Curry's Observations, p. 71.

ponding mode of treatment, such as the *traumatica* and *venenata*, may, perhaps, be safely and properly admitted; but, when distinctions are made upon a presumption only of the knowledge of causes; and yet lead to a specific practice, as in the *sanguinea* and the *serosa*, they should not, I think, be received without a very careful examination of the grounds on which they are made." The doubt of the propriety of this division is, we think, rendered more evident by the fact, that blood and serum are both effused in many cases: we often find the ventricles filled with a sero-sanguineous fluid; and probably the same irritation of the arachnoid that in one degree gives rise to exhalation of only serum, will, on being increased, give occasion to sanguineous effusion. The best distinction would perhaps be, into that from rupture of blood-vessels, and that from exhalation of fluids from their capillary extremities, developed in the arachnoid membrane, consequent on irritation of them: the former giving rise to the sudden disease, and ordinarily depending on ossification of the arteries; the latter coming more slowly, and depending on the common causes of irritation, and attacking persons far less advanced in life than the former. We may add, that effusions of pure blood in the ventricles originally never takes place: when it is present in them, it will be found to have made its way there from the substance of the brain, where it was poured out.

With regard to the *apoplexia hydrocephalica*, or *hydrocephalus internus* of Cullen, Dr. Cooke says, "I wish to observe, that it appears to be a complaint very different from the true apoplexy, both as to its nature and treatment, and ought to be considered as a distinct and separate disease." Yet, we think, it would be very difficult to say in what the precise difference between this and *serous* apoplexy consists. They both seem to depend on, either irritation of the exhalants of the arachnoid membrane, or venous congestion, preventing the due absorption of the serum ordinarily exhaled; and hence, from these causes respectively, acute and chronic hydrocephalus.

After passing in review the classifications of the nosologists, and the opinions of the greater part of the most eminent writers, Dr. Cooke concludes with saying,

"On the whole, if we admit the distinction of apoplexy into

the sanguineous and serous, I think, we must also admit that the serous apoplexy very seldom occurs; and I am of opinion that, even in very old persons, of leucophlegmatic habits, pale countenance, small pulse, and other marks of serous apoplexy, if the disease come on suddenly, it ought to be considered as probably arising from an effusion of blood within the cranium."

We may add to this, that when the apoplectic attack has been preceded for some days by head-ache, vertigo, &c. that it will commonly be found to have arisen from serous or sero-sanguineous exhalation into the ventricles of the brain, or on the surface of its hemispheres. The *sudden* disease most commonly attacks old people; and, as we before remarked, it appears to result, in a great proportion of cases, from rupture of a blood vessel, commonly dependant on ossification of its parietes.

The *diagnosis* and *prognosis* are next considered. On the former point, such an abstract as we could here adduce of the author's discussion, would not convey more useful information than what is indicated in the foregoing observations. On the latter point, the author protests against the decision of some writers, that the strong disease itself is always fatal: he has himself seen more than one instance of recovery from it; and he inserts a communication from Mr. Astley Cooper on this subject, who says:

"The dissections which I have made of cases of apoplexy, and extravasations of blood upon the brain from accident, have led me to the belief that the effused blood never becomes absorbed, but that the brain gradually acquires the power of bearing its pressure; and that thus the symptoms which are produced at the first moments of general extravasation gradually diminish.

"I will give you instances of these extravasations. My friend and pupil, Mr. Saunders the oculist, had repeated slight apoplectic attacks for many months before his death, of which he apparently recovered; but at length he died from a sudden and large extravasation of blood into one of the ventricles of his brain. Upon examination of his head, besides the great extravasation above mentioned, several streaks of coagulated

blood were found in the pons varolii, and in the cerebellum, the colour of which was so different from the recent extravasations, as clearly to indicate that they had been long effused.

“The other example is, that of a gentleman who fell from his horse, struck his forehead violently, and was taken up comatose. He recovered from these symptoms, and appeared to be well, excepting that he had a slight defect in vision. Three months afterwards, from improvident conduct, he brought on symptoms of inflammation of the brain, of which he died; and, upon examination of his head, a large coagulum, which I have preserved, was found deeply imbedded in the anterior lobe of the cerebrum, opposite the part at which he had received the blow, and which had the colour of blood long retained in an aneurismal sac.”

The observations of Rouchoux and Riobe, to which we have already referred, and the more recent researches of Serres and Bricheteau, present more favourable results; for they shew, indubitably, that coagula of blood are often absorbed from the brain, by the medium of the cyst formed around them, of which we have spoken. We cannot accompany Dr. Cooke through his remarks on the observations of other authors, but we shall adduce those which are the results of his own experience.

“When the pulse,” he says, “after having been slow, strong, and full, becomes quick, weak, and intermitting, especially in conjunction with other unfavourable signs, we may conclude that the disease will soon terminate fatally.

“Among the dangerous signs in apoplexy, many authors mention a dilated state of the pupil of the eye; but the contracted pupil, which I consider to be a still more dangerous appearance, has been scarcely noticed. I am of opinion, that this ought to be reckoned among the very worst symptoms of the disease. I never knew a person recover from apoplexy when the pupil was greatly contracted. My opinion on this subject is confirmed by that of Sir Gilbert Blane and Dr. Temple.

“Cold and profuse sweats are very unfavourable symptoms in apoplexy. Hippocrates thinks, that sweat coming on after difficulty of breathing, is a fatal sign, and coldness and torpor

dangerous.* Etmuller adopts this opinion, and considers this sweating as indicative of a failure of the vital powers, and not a natural perspiration.† When we have reason, from the sudden accession of the disease, and other circumstances, to think that a considerable vessel in the brain is ruptured, we may almost despair of the patient's recovery."

Dr. Cooke makes Hippocrates state, that when fever comes on in patients of this disease, a solution of it takes place, (ἀντοισι (αποπληκτικοῖσιν) δὲ πάλιν τοῦτοισιν ἦν πυρετὸς ἐπιγενήται, λύσις, *Coc. prenot.*;) but this judgment is so contrary to what modern experience seems to indicate, that we are disposed to think Hippocrates meant this to relate to paralysis, to which it is more evidently true; and we have already shewn that he speaks of apoplexy of the tongue, the arm, the leg, &c.; and, what favours the notion we propose, is, the cause Hippocrates assigns for the cases alluded to in the passage cited by Dr. Cooke, just transcribed, being that to which the ancients generally attributed paralysis. In the sentence immediately preceding that above cited, they are designated as dependant ἐπὶ τῇ δυσφορίᾳ: and, he adds, *Ta exaiphenes apoplectika lelymenos epipyretenanta chrono, olethria.*

With respect to the hopes that may be entertained of recovery at different periods of the disease, Dr. Cooke says,

"If the strong apoplexy has continued for even half that time [one day], I believe it almost always terminates in death. If the patient does not shew symptoms of amendment soon after the employment of the most powerful means, a fatal termination of the disease may be expected.

"If the pulse sink and intermit, if coldness of the extremities, with cold clammy sweats, come on, and the power of respiration greatly diminish, we may predict that dissolution is inevitable, and fast approaching."

The chapter on the *treatment* of apoplexy is another of which we are unable to give an useful abstract. The author himself much favours the use of blood-letting, both as a preventive and remedy: blood should, he says, in the strong dis-

* Ἐν τοῖσιν ἀποπληκτικοῖσιν ἐπὶ τῇ δυσφορίᾳ τῷ πνεύματος ἰδρῶς ἐπιγενόμενος, θανάσιμον, ψύξις δὲ καὶ ναρκώσεις, πονηρόν. *Coc. Pren.* p. 485.

† Etmuller, p. 907.

ease "be evacuated speedily and freely, generally and topically;" an opinion that is supported by almost all those authors who have written from due reflection on actual observations: but Dr. Cooke is far from advising the use of it to such an extent as some others have done. He says, "I would not venture to persist in the abstraction of blood; if, after free and repeated bleedings, there was no apparent advantage; and, *a fortiori*, if symptoms of debility should supervene." A case related by LANCISI outvies, however, any thing that even the most zealous favourers of blood-letting have of late adduced. A man, he says, fifty years of age, who indulged in the pleasures of the table to excess, very fat, and of sedentary habits, experienced, for about a month, such a heaviness and somnolency, that he would fall asleep even when counting his money; he was evidently threatened with apoplexy, when he lost in one night eleven pounds of blood by nasal hæmorrhage; he was not weakened by it, and felt somewhat lighter. Four days afterwards, he lost four pounds more blood, and was restored by it to good health. Some cases of this kind, not less interesting, were related by Dr. Kinglake, in the last volume of this Journal.

The author then passes in review the other remedies that have been employed or proposed for this disease. None of them has given occasion to so much disputation on their propriety as emetics: we shall transcribe the conclusion of Dr. Cooke on this point.

"If I were to give an opinion on this very important question, I would say, that I think our practice in this respect ought to be guided by the particular circumstances of each case. In the strong apoplexy, there may be danger of determining too much blood to the head by the act of vomiting: I therefore would not venture to prescribe an emetic till the safer remedies had been unsuccessfully employed. Although we do not precisely know how the brain is affected during the act of vomiting; and, although we are informed by Dr. Bryan Roberston and others, that emetics may be safely given in hæmoptysis and other hæmorrhages, I think that vomiting should not be excited, in the strong apoplexy, till depletion had been tried in vain: but if, after free and repeated evacuations of blood,

both general and topical, and the administration of clysters and other revellents above mentioned, no sign of amendment should be perceptible, I would endeavour to excite the action of the *vis medicatrix naturæ*, by the exhibition of an emetic of speedy operation, such as the white or the blue vitriols. In favour of the practice, I am now venturing, under certain circumstances, to recommend, I would observe, that some instances might be given of restoration, from even the strong apoplexy, on the exhibition of an emetic; and I myself have witnessed one case of recovery from the disease in a somewhat milder form by this remedy, when bleeding, &c. had been prescribed without any good effect."

Lethargy, coma, carus, and cataphora, are then the subjects of discussion; but nothing new, in the way of distinction or nosological arrangement of those affections, is proposed by the author; and he remarks, that but little has been said about either their nature or treatment that is not to be found in the writings of the ancients.

On *apoplexia hydrocephalica*, we find hardly any thing original of importance; and the history of this affection is much less perfect than that of apoplexy. Many valuable works are not noticed, and none of those of the later German Physicians, which we referred to in our review of Dr. Cheyne's Essays on this disease. The works of Goelis, Formey, Hopfengartner, Portenschlag-Ledermayer, and several others, would have furnished some very important observations respecting the most frequent remote causes of the disease, to place in opposition to those which seem to have been, as we think, rather too exclusively regarded by most of our modern writers.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

 FOR THE ECLECTIC REPERTORY.

Further Remarks on the Case of Ligature of the Arteria Innominate. By VALENTINE MOTT, M. D.*

IN my first communication of this case in the New York Hospital Register, for 1818, it is stated, page 50, that "*the subclavian artery, internally and externally to the disease, was pervious.*" To this it may now be added, that where this artery opens into the ulcer, left from the wound of the operation, it appears not only pervious, but of the natural size, and the coats free from any diseased appearance. Externally, towards the axilla, the artery is somewhat enlarged in diameter, but exhibits no appearance of disorganization of its coats either externally or internally. About an inch from the ulcer, or just as the artery has passed between the scaleni muscles, there is an irregularly shaped elliptical opening upon its upper side, large enough to receive the extremity of the fore finger. The edges of this opening are jagged and uneven, and the surface of the artery, internally, is of a brownish yellow colour, to the extent of half an inch on the inside of the opening, and more than an inch towards the axilla. The internal coat of the artery has a rugous or puckered appearance; separated a little from the muscular coat, very friable, and evidently in a degenerated state. This opening of the artery communicates directly with the anterior extremity of the sac, which contains coagula, and upon removing these the surface of the sac is seen puckered, or thrown into a great number of little folds, giving it, at first sight, the appearance of containing a number of lobes. This account is taken from the morbid parts before me, and the preparation has been seen and examined by Dr. Post, Dr. Hosack, Dr. Stevens, Dr. Watts, and others; who have authorized me to state, that they are satisfied as to the nature of the case.

New York, 9th mo. 29th, 1820.

* Vide Eclectic Repertory, January, 1820, Vol. ix. No. 1.

New Febrifuge Plant.

In the Madrid Gazette, under the date of June 25, 1819, is announced a new febrifuge plant, known to the Indians of Quito, by the name of *chinininka* (pronounced as *chinininga*.) It is a shrub of a new genus. Dr. Joseph Pavon has named it *unanuea febrifuga*, and presented it to the Academy of Sciences of Madrid. Some experiments have already been made upon it in intermittent fever; and several well-known Physicians have obtained great success from giving a scruple of the root, in powder, every three hours. The dose is afterwards raised to half a drachm; and by this mean, the febrile paroxysms, which had resisted even the cinchona, have been prevented.—*London Med. Repository.*

Microscopical Observations on the Human Urethra. By Sir E.
HOME.

For the chief observations in this paper the author confessed himself indebted to Mr. Bauer. From these it appears that the human urethra is made up of two parts: an internal membrane, and an external muscular covering. The former is very thin, and destitute of fibres. It is thrown into folds in the collapsed state, and upon its surface are numerous orifices of glands. The latter is made up of short interwoven fibres, forming fasciculi, united by an elastic substance of the consistence of mucus. These facts shew, in the author's opinion, the fallacy of the common opinion, that the lining of the urethra consists of circular contractile fibres, and thus throws a new light on stricture; a spasmodic stricture being caused by a contraction of a small portion of the longitudinal muscular fibres, while the others are in a state of relaxation; and a permanent stricture, by the exudation of coagulable lymph, in consequence of inflammation between the fasciculæ of muscular fibres, and upon the internal membrane.

After briefly noticing what is already known respecting the structure of the corpus spongiosum and corpora cavernosa, the author proceeds to relate Mr. Bauer's observations on these parts. The cellular structure of the corpora cavernosa is made

up of very thin membranous plates, very elastic, and so connected as to form a trellis-work, the edge of which is attached to the elastic ligamentous substance which surrounds them, and which forms the septum between them. The structure of the corpus spongiosum was stated to be similar to that of the corpora cavernosa, except that the parts are formed on a smaller scale, and that there are no muscular fibres in its ligamentous elastic covering. These observations were illustrated by several beautiful drawings made by Mr. Bauer.—*Lond. Med. and Phys. Journal.*

Ramsay on Pyroligneous Acid.

NEARLY two years since, we stated in this Journal some facts respecting the peculiar antiseptic powers of the *pyroligneous acid* in respect to animal substances, and the probability of its being applied to useful purposes, especially in the equipment of fleets. Mr. WM. RAMSAY, in a letter to the Editors of the Edinburgh Philosophical Journal; (see No. V.) has made some experiments on this subject, which still further shew its claims to more attention than it has yet excited.

“On the 10th of July last, a number of herrings were cleaned in the common manner, and, without being salted, were immersed for three hours in distilled pyroligneous acid, of the specific gravity of 1.012. On withdrawing the fish from the acid, they were considerably softened, and had not the firmness of fish taken from a pickle of common salt. In this state they were hung on a rod to dry in the shade, were frequently examined, and, though the months of July and August were very hot, the herrings had not the least sign of putrefaction: on the contrary, the smell was quite wholesome, although joined with the flavour arising from having been steeped in this acid. On broiling one of the herrings, the empyreumatic smell was strongly developed, and the fish were by no means agreeable to those who are not accustomed to this flavour; but, after being kept for upwards of six months, they remained in a state of complete preservation. From experiments made since, I find that pyroligneous acid of the specific gravity of 1.012, is too strong for the immersion of fish for so long a period

as three months, but that simply dipping them in acid of the above strength is sufficient for their preservation, if they are afterwards dried in the shade; and, on boiling herrings done in this manner, they are very agreeable to the taste, and had nothing of the disagreeable empyreuma which those had which were steeped for three hours in the acid.

“ Although it is thus evident that fish may be preserved without the intervention of salt, yet mankind are so accustomed to the habitual use of salt in the preservation of food of almost every species, that my next experiments were made with the view of uniting these antiseptics in the curing of provisions.

“ A number of very fine haddocks were cleaned, split, and slightly sprinkled with salt for six hours. After being drained, they were dipped for about three seconds in pyroligneous acid, and then hung on a spit in the shade for eight days before being used. On being broiled, the fish were of an uncommonly fine flavour, delicately white, and were equal to what are called Finnan haddocks, which are so much esteemed. As a comparative experiment I allowed one of the fish to remain twelve hours in the pyroligneous acid: on withdrawing it, it was soft and tender, which shewed that the acid had begun to decompose the muscular fibres of the fish. On broiling it, it had the same bad qualities of the herring which were too long steeped in the acid, and was unpalatable, from the strong empyreuma and acid taste.

“ Herrings were cured in the same manner, and with the same success, by being first slightly cured with salt, drained, and then immersed in the pyroligneous acid. After being dried in the shade for two months, the quality and flavour were equal to those of any red herring I ever tasted, and the fish retained the shining and fresh appearance they had when taken from the sea.

“ When these experiments were begun, in July 1819, the manager of the works in which I am interested dipped a piece of fresh beef in pyroligneous acid of the specific gravity of 1.012. It was not immersed more than one minute: it is now in my possession, March 4th, 1820, and is as free from taint as on the day when the experiment was made, in July: no salt

was here used. He at the same time dipped a piece of beef in pure vinegar, the specific gravity of which was 1.009: he informed me that on the 18th of November last it was perfectly free from taint, when, being broiled, it had a very agreeable sub-acid taste. From the latter circumstance, it appears that vinegar possesses, in a certain degree, the same antiseptic quality as the pyroligneous acid. Since I was informed of this circumstance, I have cured some haddocks with pure vinegar: they are entirely free from taint, but when cooked they had an insipid taste, from the want of the slight saline taste to which we are accustomed.

“ From observing the change which takes place on fish when too long immersed in pyroligneous acid, it appears that the acid, when too strong, acts as a solvent of the fish, as the texture is not only shortened, but fibrous matter was diffused through the liquid, and the bulk of the fish was diminished. But when fish are only dipped in the acid, no diminution of bulk takes place.”

It yet remains to be ascertained whether or not animal matters will be equally well preserved when *packed up*, after having been submitted to the process above described.—*London Medical and Physical Journal*.



London Institution for the Benefit of the Widows of the Medical Officers of the Army.

THE anniversary dinner of the Institution for the Benefit of the Widows of the Medical Officers of the Army, founded by Sir JAMES M'GRIGOR, was held at the Thatched House Tavern, St. James's street, on the 26th of May, Dr. Borland, Inspector of Hospitals, in the chair; when one hundred gentlemen sat down to dinner. Among the guests were Dr. Latham, President of the College of Physicians; Sir David Dundas, Master of the College of Surgeons; the Master of the Worshipful Company of Apothecaries; Sir Everard Home; Sir Matthew Tierney; Mr. Crampton, Surgeon General of Ireland; Drs. Baillie, Cook, Sutton, and Drs. Johnson and Beattie, of the Royal Navy; Professor Brand; Messrs. Wilson, Carpue, Brooks, Brodie, Alexander, &c.

The objects of the Institution, now in its fifth year, having been most fully obtained, payment of the pensions to widows commences in January next. The Society for the Widows of Medical Officers of the Army having succeeded beyond all expectation, it is now in contemplation to establish a subsidiary Society for Orphans, with a fund for benevolent purposes.

There are now 590 subscribers, of whom 412 are unmarried, and the capital stock of the Society amounts to 17,169*l.* 16*s.* 7*d.* 3 per cents, reduced, producing with the subscriptions, nearly 2600*l.* a-year; being an increase, after filling up vacancies by death and otherwise, of ten members, and of 4000*l.* 3 per cents. since the last annual meeting.—*Edinburgh Medical and Surgical Journal.*

Extract from a letter from Dr. SANDERS, Lecturer on the Practice of Physic, Edinburgh, to Dr. DUNCAN, junior.

It will not, I trust, be unacceptable to you to know precisely the results of my anatomical inquiries on Tetanus,—results, indeed, connected with the whole tribe of spasmodic diseases.

Professional avocations do not allow me at present, to enter into any elaborate detail or discussion; permit me, therefore, to state briefly the general facts.

1. If any muscle, voluntary, or involuntary, is affected with spasm, and, during, this affection, the person dies, on examination it is found, that the nerves, which supply the spasmed muscle, are covered with turgid red vessels at their visible origins, or where they appear to set off from the brain, medulla oblongata, or spinal marrow.

This turgescence, and the effects of turgescence, are in the ratio of the degree and duration conjointly of the spasm or convulsion.

The turgid vessels in every obstinate and severe case, may be traced into the substance of the spinal marrow, by the sides of the striæ, which seem to be the continuations of the nervous filaments; as also along the nervous cords, through their sheaths formed in the dura mater.

The position of the body after death has no appreciable in-

fluence on these appearances: they are observed anteriorly or posteriorly, in the loins, thorax, cervix, or within the skull, bearing strict relation to the parts which have evinced spasmodic action.

But the nerves serving the muscles which have not laboured under spasm or convulsion, are free from turgid vessels.

2. If the tetanic affection is confined to the jaw, certain nerves arising from the *tuber annulare* and *medulla oblongata*, are found in the state above described.

But if the tetanic affection involves the whole inferior extremities, and the trunk of the body, as well as the jaws, then the origins of the nerves from the *tuber annulare* to the *cauda equina* are covered with turgid red vessels.

In short, the nerves exhibiting such turgescence at their origins, correspond in number and situation with the muscles which have exhibited inordinate contraction. I have conducted this investigation for about sixteen years, and have not met with one exception.

Some are of opinion, that I maintain, that the spinal marrow, its nerves and membranes, are always affected with turgid vessels in tetanus: this is incorrect. I have examined cases of trismus, in which the spinal marrow, its membranes, and nerves, were almost entirely sound, from the atlas to the lumbar vertebræ; in these instances, however, not only the origins of the nerves at the *medulla oblongata*, but the *medulla* itself, was inclosed with a close net-work of turgid red vessels.

There are many other appearances within the cranium and spinal canal more or less connected with spasms and convulsions; but those, which I have here related, are uniform, and accordingly constitute, we have reason to believe, an essential part of these diseases.

Morbid changes in the organization, as of substance of the brain, and spinal marrow, or in their envelopes, belong to another order of maladies, and when present in those affected with spasms, the symptoms always indicate complication.

If you deem this communication deserving, let it have a place in the Journal.—*Edinburgh Medical and Surg. Journal.*

University of Pennsylvania.

THE Medical Lectures will commence, as usual, on the first Monday in November:

Anatomy,	Dr. Physick.
Practice of Physic, &c.	Chapman.
Midwifery,	James.
Chemistry,	Hare.
Surgery,	Gibson.
Materia Medica and Pharmacy,	Coxe.

Pennsylvania Hospital.

Patients remaining in the house, April 24, 1819,	197
Admitted from that time to April 22, 1820,	748
	<hr/> 945
Of these the number cured is	428
relieved	81
removed	54
women safely delivered	46
infants born and well	44
discharged, disorderly	8
eloped	23
dead	52
remain	209
	Total, <hr/> 945

Obituary.

DIED, September 6, 1820, Doctor ROBERT WILLIAM WORTHINGTON, aged twenty-six years.

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